5-2016

Effects of the Duration and Benefit Level of Unemployment Insurance during the Great Recession: Evidence from Kentucky Administrative Data

Shan Jiang
Clemson University, shanj@g.clemson.edu

Effects of the Duration and Benefit Level of Unemployment Insurance during the Great Recession: Evidence from Kentucky Administrative Data

---

A Dissertation
Presented to
the Graduate School of
Clemson University

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy
Applied Economics

---

by
Shan Jiang
May 2016

Accepted by:
Dr. Curtis Simon, Committee Chair
Dr. Chungsang Tom Lam
Dr. Scott Barkowski
Dr. Babur De los Santos

Abstract

Both the duration of and generosity of Unemployment Insurance (UI) payment increased substantially during the Great Recession. This paper attempts to quantify the separate effects of two of the changes affected by the program during this process: multiple extensions of the maximum benefit spells, and the increase in the dollar benefit levels, using administrative data from Kentucky for the period January 2006 through December 2011.

In the first chapter, I estimate Rothstein (2011) specifications, and introduce a variety alternative hazard models to estimate the effect of the multiple extensions on reemployment. My estimates suggest that extensions of UI spells lowered monthly exit hazards by 1 percent, and extended the unemployment duration by 0.4 weeks. My estimates imply that a fair fraction of the persistent increase in long-term unemployment after the Great Recession is due to a decline in UI recipients' search efforts.

In the second chapter, I identify the effect of the benefit level in the Regression Kink Design (RKD) using kinks in the schedule of UI benefits, and find the elasticity of unemployment duration with respect to the benefit level ranges from 0.2 to 0.6. My results suggest that given the massive extensions of the benefit weeks, unemployed workers responded to benefit level changes less than without the multiple extensions of benefit weeks.

Dedication

I dedicate my dissertation work to my loving parents and my husband, who have been a constant source of support and encouragement during the challenges of graduate school and life.

Acknowledgments

My thanks and appreciation go to Professor Curtis J. Simon for persevering with me as my advisor through out the time it took me to complete this research and write the dissertation. The process of finishing my dissertation was one of the most important and formative experiences in my life. I am grateful as well to J Michael Jones, the Chief Economist Governor's Office for Policy Research, for obtaining the data and help with institutional details that made it possible for me to complete my dissertation.

The members of my dissertation committee, Scott Barkowski, Babur De los Santos, and Chungsang Tom Lam, have generously given their time and expertise to better my work. I need to express my gratitude and deep appreciation to them for their contribution and their good-natured support.

I am grateful for the support and advise from other faculties in the John E.Walker Department of Economics. I must acknowledge as well the many friends, colleagues, students, teachers who assisted, advised, and supported my research and writing efforts over the years.

Table of Contents

List of Tables

List of Figures

Chapter 1

The Effect of Extended UI Eligibility on the Duration of Unemployment

1.1 Introduction

Over the course of the Great Recession, the national unemployment rate rose from 6% in the middle of 2008 to almost 10% in the latter months of 2009. Although the recession ended officially in June 2009, unemployment remained stubbornly high, and the unemployment rate did not fall below 9% until January 2012, over two years after the Great Recession ended. Even more strikingly, the long-term unemployment rate[1] reached much higher levels (45.5%, the historic high) which persisted much longer in the Great Recession than in any previous period since the late 1940s. While the unemployment rate slowly receded after the peak of about 12%, the long-term unemployment rate remained staggeringly high at more than 39%[2], and the mean durations of unemployment reached 40 weeks (historic high) in June 2010, one year after the Great Recession ended as indicated in Figure 1.1. The persistence of high long-term unemployment is a cause for concern regarding the multiple extensions of unemployment insurance (UI) that provided up to 99 weeks of unemployment

[1] The long-term unemployment rate is defined as the share of the unemployed who have been out of work for 27 weeks (six months) or more.

[2] The highest level in the history was 26%

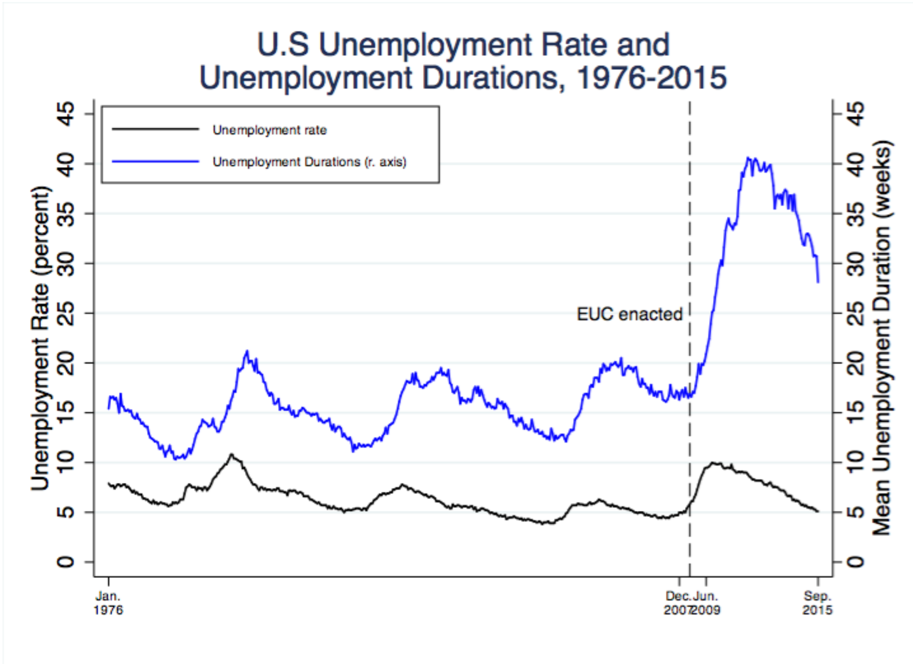


Figure 1.1: U.S. Unemployment Rate and Unemployment Durations

benefits to qualified unemployed workers in most states.

During normal periods, UI programs in the United States (US) provide temporary benefits for eligible workers for up to 26 weeks. In times of high unemployment, the Extended Benefit (EB) program extends the benefit period for another 20 weeks. In June 2008 (the wake of the Great Recession), Congress created the Emergency Unemployment Compensation (EUC) program. Including the standard 26 weeks of regular State UI benefit, 20 weeks of EB, and a series of extensions, the total weeks of available unemployment benefits reached a maximum of 99 weeks for an eligible unemployed worker in late 2009 and continuing into 2012.

From Figure 1.2, it is clear to see that the Kentucky's experience was similar to the rest of US, but with higher unemployment rates than the average national level over the period, especially during the Great Recession and in its aftermath. In addition to the extension of UI periods, weekly benefit levels also increased in Kentucky over the course of the recession. Unlike most states in the US, the weekly benefit payout eligibility in Kentucky is independent of the number of weeks of benefit eligibility. This setting allows me to distinguish between UI benefit level and the number of weeks of UI entitlement in terms of their effects on unemployment durations.

'

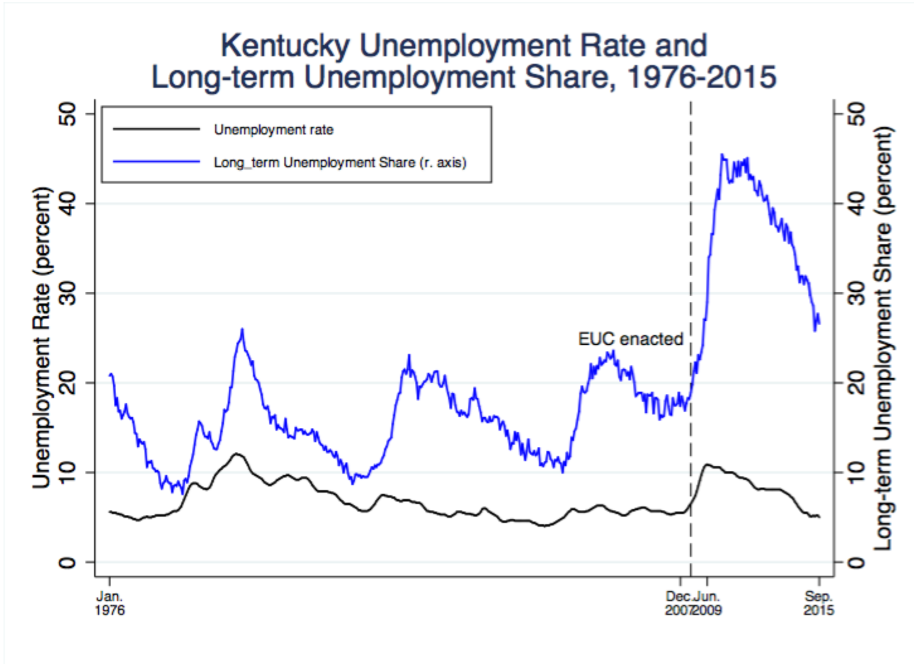


Figure 1.2: Kentucky Unemployment Rate and Long-term Unemployment Share

This paper presents new estimates of the effect of multiple extensions of maximum UI periods in the UI program implemented during the Great Recession (which began in December 2007 and ended in June 2009) using data from Kentucky. Within the broad empirical literature devoted to examining effects of unemployment insurance, few studies have had the approaches to examine the effects of the policy of multiple extensions such as those implemented in the US during the Great Recession. My paper follows in the line that does investigate the effect of extensions of UI periods during the Great Recession. Studies include Rothstein (2011), Farber and Valletta (2013) and Fujita (2011), but all of these studies use the Current Population Survey (CPS) data, which lack crucial information on claimants' eligibility for benefits, and assume that all identified eligible unemployed workers receive UI benefit payments. Farber and Valletta (2013) shows that only half of unemployed individuals who are identified as UI recipients in these studies reported receiving UI income, whereas around 40% of those who are identified as not eligible for UI reported UI income.

The detailed administrative data for the state of Kentucky, I use flexible specifications to accurately capture variations in periods of UI across time and individuals. During my sample period, the EUC program, which extended the UI repeatedly, paused on three occasions (April 4, May 50 and December 19 2010). By using the detailed administrative data, I also capture the effect of the sudden removal of UI benefit. I find that the reduction of UI benefits encouraged recipients to search for jobs intensively and increased the probability of finding jobs. My estimates suggest that implementing the multiple extensions of UI periods decreases exit hazards for jobs by 1 percent, which is twice the estimates from studies using CPS data (Farber and Valletta (2013), Fujita (2011) and Rothstein (2011)). My estimates suggest that each additional week of UI eligibility increases the duration of the unemployment duration by 0.4 week, which is in line with the previous estimates in the US such as Card and Levine (2000) Meyer (1990) and Landais (2013). Given the maximum of 53 weeks of UI extensions, the average unemployment duration could potentially increase by 21 weeks, which implies that a fair fraction of the persistent increase in long-term unemployment after the Great Recession is due to a decline in UI recipients' search efforts.

My most general specification suggests that the extended benefit coverage lengthens unemployment durations , and the disincentive of UI extension dominates at most levels of remaining benefit weeks. The marginal disincentive effect of the extension increases if extensions happen at the end of a UI spell. Moreover, longer entitlements increase reemployment hazards for short-term unemployed claimants, while discouraging long-term unemployed claimants from exiting UI to new jobs.

1.2 Studies of the effect of Unemployment Insurance on the Duration of Unemployment in the U.S.

A broad empirical literature is devoted to examining the effects of UI on the length of unemployment spells. There is evidence from randomized social experiments in the U.S. show that the generosity of UI benefits do change the incentives and affect the speed with which unemployment workers leave the unemployment insurance. The most recent studies which investigate the effects of the multiple extensions of the unemployment insurance on the labor market use matched individuals across months of Current Population Survey (CPS) data, and conclude that the effect of extensions to the UI period is small, with the estimates implying that only a small fraction of the persistent increase in unemployment after the Great Recession was due to a decline in unemployed workers' search efforts. Rothstein (2011), Farber and Valletta (2013) and Fujita (2011) adopt discrete-time hazard specifications to model the conditional probability that an unemployment spell ends at certain duration, and use competing risk models to analyze the transition out of unemployment to new jobs, and exiting unemployment to leave the labor force.

The number of benefit weeks vary across individuals due to their eligibility for UI extensions. In addition, since UI was extended when the labor market worsened, the total number of benefit weeks available to each person varied with labor demand condition as well. Studies use different strategies to identify the different components of the variation in benefit weeks by assuming that the variations are exogenous to the unobserved determinants of the unemployment hazard. Rothstein (2011) computes eligibility for benefits in each week between the time of being unemployed and the time of initial interview, and simulates remaining benefit durations for each of them. Instead of including simulated available benefit weeks between the time of displacement and the initial CPS interviews, Farber and Valletta (2013) uses two dummy variables to capture the gradual extensions of UI: the indicator for availability of extended benefits, EB_{it}, and the indicator for whether individual i is in the last month of availability of benefits, $last_{it}$. Fujita (2011) uses samples which cover the periods between 2004-2007 and 2009-2010, and excludes the observations for 2008 when the EUC was enacted. The data prior to 2008 are used to infer the shape of the hazard functions when there was no UI. Nevertheless, these strategies fail to capture the dynamic changes of qualified benefit weeks for each claimant.

Empirical Specification	*Data and Identification*	*Findings*
Meyer (1990) and Katz and Meyer (1990). Hazard model for exit from unemployment with nonparametric baseline hazard and variables for benefit level, and spikes of time until benefits exhaust. Includes controls for state unemployment and past wages, and state indicator variables.	U.S. CWBH data for 13 states 1978-1983. Identification from differences in benefit schedules across states and changes in benefits and potential duration over time.	Elasticity of duration with respect to the benefit of 0.8, and with respect to potential duration of 0.5
Card and Levine (2000). Hazard models of exit from unemployment receipt.	U.S. administrative data for New Jersey. Examines program that offered 13 weeks of 'extended benefits' for 6 months in 1996.	Elasticity of duration with respect to potential duration of 0.1
Rothstein (2011). Logit model for exits from unemployment to employment, and exits out of the labor force.	Current Population Survey (CPS) May 2004 - January 2011. Use the haphazard roll-out of the EUC and EB programs during the Great Recession to identify the partial equilibrium effects of the UI extensions.	UI extensions reduced the average monthly reemployment hazard of unemployed displaced workers by 0.5 percentage points, and reduced the monthly labor force exit hazard by 1 percentage points.
Farber and Valletta (2013). Probit model for exits from unemployment to employment, and exits out of the labor force.	CPS for the periods 2000-2005 and 2007-2012 Base on individual variation in benefit availability, conditional on state economic conditions and individual characteristics to identify the UI effect	Extended UI increased the overall unemployment rate by about 0.4 percentage points.
Fujita (2011) . Logit model for exits from unemployment to employment, and exits out of the labor force.	CPS male workers for the periods 2004-2007 and 2009-2010	Extended benefits have raised male workers unemployment rate by 1.2 percentage points.

Table 1.1: Summarize studies of unemployment durations that are affected by UI.

1.3 Emergency Unemployment Compensation Program

Unemployment insurance (UI) provides payments to people who apply for benefits and are eligible to receive them due to involuntary job loss. To determine whether unemployed individuals are monetarily eligible for the UI benefit, Kentucky has legislated tests which must be satisfied. To maintain eligibility for benefits, unemployed UI recipients must continue searching for a new job. Recipients can generally receive up to 26 weeks of benefits under the existing UI, and an additional 20 weeks of benefits from the Extended Benefit program (EB) when unemployment rates is over 8%.

To alleviate the severe negative employment shock during the Great Recession from December 2007 to June 2009, Congress created the Emergency Unemployment Compensation Program in June 2008, which offered an extension of UI benefits fully funded by the Federal government. After a series of extensions, including regular 26-weeks State UI benefit and 20-weeks EB, the total weeks of available unemployment benefits reached a maximum of 99 weeks for an eligible unemployed worker.

Table 1.2 shows the evolution of EUC program during and after the recession. The June 2008 legislation made 13 weeks of EUC benefits available to unemployed individuals who exhausted regular benefits after May 1, 2007, and claimed regular UI after November 5, 2006. The EUC program was expanded in November 2008. That expansion increased the EUC benefits to 20 weeks and added a second tier of 13 additional weeks of benefits in states with unemployment rates above 6 percent. Additional expansions in February 2009 and November 2009 increased tier II benefits to 14 weeks and added tier III, which provided 13 additional weeks of benefits in states with unemployment rates above 6 percent–and tier IV, an additional 6 weeks in states with unemployment rates above 8.5 percent. These EUC extensions created benefit durations as long as 53 weeks in some states (including Kentucky). On three occasions (in April, June, and November 2010), Congress allowed the program to expire. Each time, Congress eventually reauthorized it retroactive to the previous expiration date, but this reauthorization took 7 weeks following the June expiration. During the expiration, claimants were covered by EB if they would have been in EUC. In Figure 1.3, I present the state unemployment rates and the dates of each EUC extension. It is clear to see that the series extensions were not correlated with the change of the unemployment rates.

		# of benefit weeks under each Tier				
Legislation Approved	Weeks Covered	I	II	III	IV	total(UI, EB)
6/30/2008	7/6/2008-3/28/2009	13				39
11/21/2008	11/23/2008-3/28/2009	20	13			59
2/17/2009	2/22/2009-12/26/2009	20	13			79
11/6/2009	11/8/2009-12/26/2009	20	14	13	6	99
12/19/2009	12/27/2009-2/27/2010	20	14	13	6	99
3/2/2010	2/28/2010-4/3/2010	20	14	13	6	99
4/15/2010[R]	4/4/2010-5/29/2010	20	14	13	6	99
5/30/2010 – 7/18/2010		*EUC Lapsed*				46
7/22/2010[R]	5/30/2010-11/27/2010	20	14	13	6	99
11/28/2010 – 12/12/2010		*EUC Lapsed*				46
12/17/2010[R]	12/19/2010-12/31/2011	20	14	13	6	99
12/23/2011	1/3/2012-3/6/2012	20	14	13	6	99
2/22/2012	3/6/2012-1/2/2013	14	14	9	10	93

Note: Kentucky triggered Extended Benefits (EB) on April 19, 2009, and the total number of benefit weeks subsequently increased by 20 weeks.

[R]Retroactively funds EUC.

Table 1.2: EUC Legal Changes

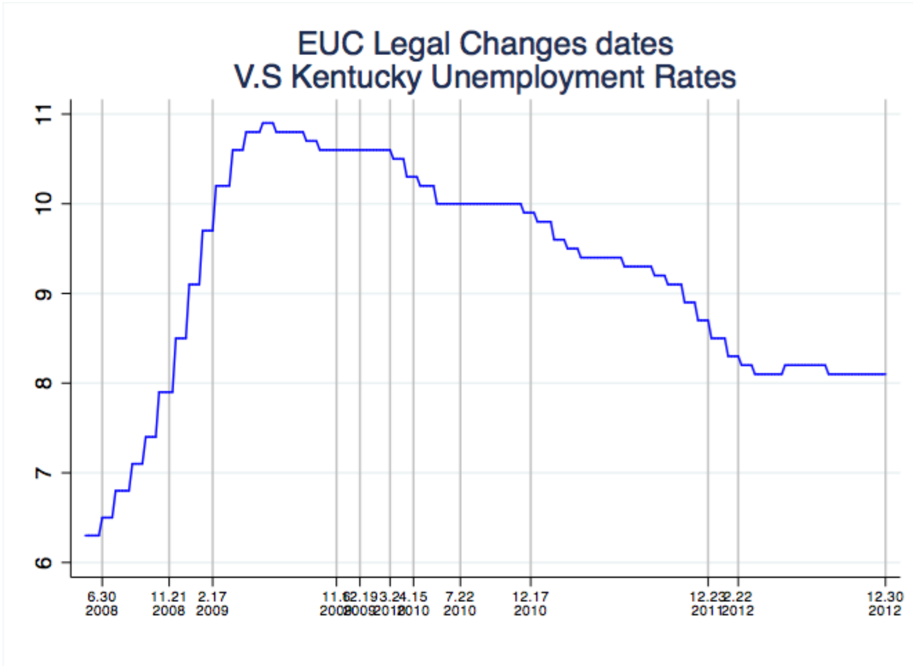


Figure 1.3: EUC change dates

1.4 Data

In this paper, I use data drawn from the Kentucky Office of Employment and Training [3]. The administrative data contain quarterly records for each individual filing for UI compensation from January 2006 through December 2011. The information relevant for my study includes the number of weeks for each claim; claimants drew benefit from which UI program; the benefit amount for which each claimant was eligible; the actual amount of benefit they drew; base-period earnings, and demographic characteristics such as age, gender, ethnicity, employer NAICS code, and years of schooling.

In addition, I have another important resource for this study is the quarterly wage files, which cover UI recipients in my sample from 2005 through 2013. The wage file contains detailed information about these UI claimants before and after they were in the UI programs, including earnings both before and after their UI spell and major industry sectors. Unlike most of the administrative data used in previous research, merging the data file helps me to track individuals into their first post-UI employment spell and permits me to calculate individuals' unemployment durations and examine

[3]The data were randomized and anonymous without personal identifiers.

the effects of UI not simply on the probability of exiting UI, but on the probability of finding employment.

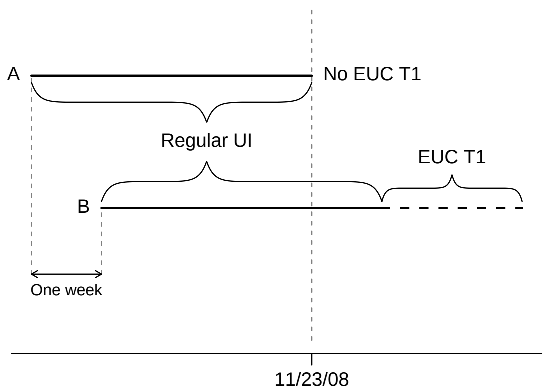
Figure 1.4(a) and (b) explain how a particular change of policy would affect individuals who filed UI claims in different time. The law used in this example was approved in November 21, 2008, and created the second tire (13 more benefit weeks) in EUC and added 7 additional weeks, beyond the original 13 weeks, into Tier I. The law covered unemployment from December 23, 2008 to March 22, 2009. Subfigure(a) illustrates the situation that claimant A filed a UI claim one week earlier than claimant B and exhausted the regular 26-weeks UI right before the law enacted. Claimant A was not qualified to be extended into EUC Tier I. In subfigure(b), claimant C started the UI one week earlier than claimant D, and hence he transitioned into Tier II before the end date (March 22, 2009) of the law. Figure 1.4(c) presents the evolution of changes of remaining benefit weeks for two people in my sample during the covered UI spell. For people who started the UI claims at various dates, they would be eligible for different amount of extensions (and reductions). Yet their changes in the remaining benefit weeks happen at the same week when the new policy is announced.

Table 1.3 reports summary statistics of variables for the analyzed sample. The sample includes 81,772 displaced workers who files 117,524 UI claims.[4] Just under 5% ($=\frac{5786}{117524}$) of claims were filed before EUC was authorized and were not eligible for the extension. Among those claims filed after the EUC legislation, 17.76% of claims were extended from state UI to EUC, and 8.47% of EUC claims were extended by EB with potential 99 benefit weeks.

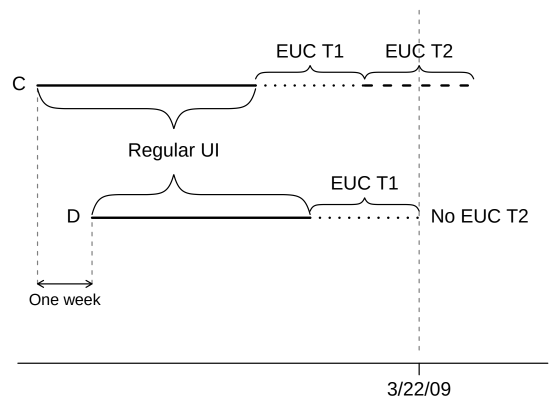
Before EUC was implemented, unemployed individuals had average UI durations of 10.4 weeks, while qualifying for 26 benefit weeks.[5] After EUC was enacted, individuals still had much longer UI durations – 29.32 weeks, yet expected 31.92 benefit weeks on average. The exhaustion rate, defined as the share of claims that exhausted all eligible benefit weeks, decreased largely after EUC was enacted. However, the rate of exit only decreased 3% after the extension.

[4]In analyzing the effect of extending UI periods, I use all observations in my sample. However in my analysis of the effect of UI benefits, I exclude observations with censored unemployment durations and wrongly recorded base-period earnings.

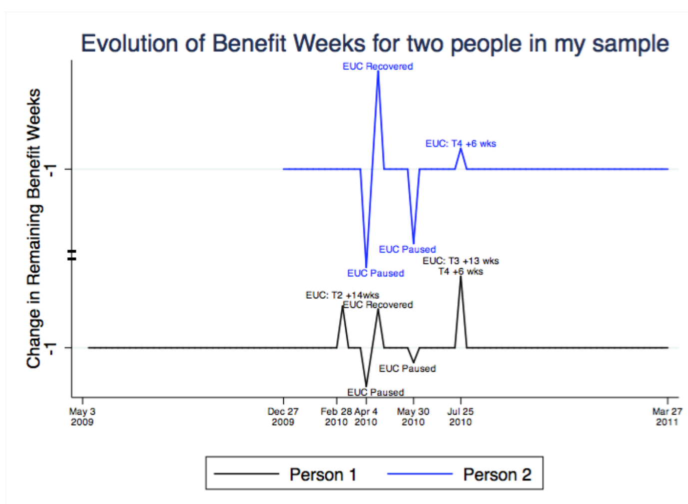
[5]In Kentucky, the number of benefit weeks of each UI claim is the same across qualified individuals. As long as unemployed workers are eligible for state UI, the number of benefit weeks is 26 for them.



(a)



(b)



(c)

Figure 1.4: Examples of extension of UI spells

Sample for the study into the extension of UI periods

	Before EUC	After EUC
# Claims filed	5,786	111,738
Share of state UI	100.00%	73.77%
Share of EUC	0.00%	17.76%
Share of EB	0.00%	8.47%
Average of total benefit weeks used	10.40	29.23
Average of anticipated weeks of benefits	15.60	31.92
Exhaustion Rate	28.02%	12.60%
Exit Rate for jobs	70.53%	67.40%
# of Sampled unemployed	81,772	
# of Claims	117,524	

Table 1.3: Summary Statistics I

1.5 Empirical Specifications

To isolate unemployment duration based on multiple extensions to UI, Rothstein (2011) develops a model by assuming that the wage is exogenous and constant across different individuals, and agents choose their search efforts based purely on the number of benefit weeks remaining, denoted as d. Rothstein (2011) presented three propositions in his paper [6]: First, as workers approach their UI benefit expirations, search intensity rises. Second, Individuals who are running out of benefits exert higher search efforts than individuals who are at the beginning of their unemployment spell with a lot of benefits still available. Thus, extending the UI period will reduce search efforts and the probability of exiting from UI to the labor market, which is known as the disincentive effect or moral hazard effect.[7] Furthermore, the policy requires that only if unemployed workers continue their job search and exert search efforts higher than some given level, then they are eligible to receive the insurance benefit across time. Based on this setting, the third proposition in Rothstein (2011) is that extensions of unemployment benefits extend unemployed workers' job search —because searching for work is a requirement for UI claimants to be eligible for the extension—and therefore, multiple extensions could possibly increase reemployment rates by lengthening the search time for

[6] I explain and prove them in details in the Appendix

[7] As indicated in Chetty (2008), given plausible parameterizations of the shape of the search function, the reduction has higher effects on individuals who have exhausted their regular benefits than on those who are still receiving their regular benefits.

unemployed claimants. Combining this with the disincentive effect of the UI, the net effect on the probability of exiting from UI to jobs is ambiguous, as predicted in Rothstein (2011). In the following two subsections, I will discuss specifications of Rothstein (2011) and mine, respectively, and test the three propositions presented above.

1.5.1 Replications of Rothstein (2011)

In this section, I estimate the reduced-form specification hazard model in Rothstein (2011) to assess predictions in the job search model, investigating the effects of multiple extensions to UI periods on the duration of unemployment spells. The dependent variable y_{it} is defined as whether a claimant exit from UI into employment at week t.

Given the importance of time-dependent covariates and censoring in the data, hazard models are widely used in the literature. Since employing a parametric assumption about baseline hazard can lead to inconsistent hazard estimates when the assumed shape of the baseline is incorrect[8], Meyer (1990) extensively discussed the use of reduced-form hazard models (e.g., the Cox proportional hazard model) which non-parametrically estimate the baseline hazard.[9] In this section, logit models use binary dependent variables and hence are also a reduced form discrete time hazard model. Logit models are used to estimate the weekly hazard of exiting UI for jobs h_{it}.[10]

Rothstein (2011) uses different specifications to identify the heterogeneity in policy which is denoted as $Policy$ in the following equations. Assuming that the weekly hazard of leaving UI for a job h_{it} follows a logistic functional form:

$$ln(\frac{h_{it}}{1-h_{it}}) = \alpha + \Pi Policy_{it} + Control_{it} + \varepsilon_{it}, \tag{1.1}$$

In the first specification, let $Policy_{it} = T_{it}$ and represent the total number of weeks of benefits available to the individual i, which by definition is equal to the benefit weeks used, n_{it}, plus the expected number of future benefit weeks, d_{it}. The number of benefit weeks, n_{it}, is the number of weeks that unemployed person i in calendar week t has been unemployed and receiving benefits. where

[8]There is rarely theoretical support for any particular shape for the baseline hazard.

[9]For example, variations over time that are not captured in the parametric baseline would be used to estimate the hazard. When variations across observations are more than over time, the non-parametric baseline ensures the consistency of the estimates while sacrificing a small amount of efficiency.

[10]Generally, individuals are identified as exiting for work when they leave UI if they reported positive earnings at the same time. In this paper, I treat people who exit but do not find a job as right-censored records when they stopped claiming benefits from UI, which is the exactly how I treat people who exhaust their entitled benefits.

$Control_{it}$ includes M_t a set of fixed time dummies for calendar month, X_i a vector of individual-level covariates, including gender, race, education level, age, and indicators for previous industry, and B_n a flexible polynomial function to represent the baseline hazard. The baseline hazard describes the hazard for individuals with all characteristics set at certain baseline values, which serves as a reference group. By contrast, the increase or reduction in risk deviated from the baseline hazard is being called deviation hazard, which captures changes of exit hazards for a set of characteristics of interest.

The coefficient Π in equation (1.1) describes the net effect of T_{it} on exit hazard from UI to a job, which is the effect's deviation from the baseline effect. The variation in T_{it} comes from repeated extensions of the EUC. The estimate of Π measures the effect of the benefit weeks available conditional on how many benefit weeks have already been used , except for the effect of the baseline. Since the estimate of Π represents the effect of unemployment benefit extensions on exit hazards, as the model predicted, if it is negative, then the disincentive effect of the extension dominates.

The flexible polynomial function of n_{it} used in the equation (1.1) has the form as followed:

$$B_n(n_{it};\gamma) = \gamma_1 n_{it} + \gamma_2 n_{it}^2 + \gamma_3 n_{it}^{-1} + \gamma_4 I(n_{it=0}) \tag{1.2}$$

In this baseline hazard function, Rothstein (2011) allowss that there is a discontinuity at zero benefit week. The data set includes all individuals who filed the unemployment insurance program, with or without drawing benefits from the program. In my data, the probability of finding employment among workers who claim no benefit is 67%, which is significantly higher than the probability among those who claim at least 1 week benefits. The duration dependence is measured by the estimated coefficients of equation (1.2) as follow [11]:

$$\frac{\partial B_n}{\partial n_{it}}|_{n_{it}=n>0} = \hat{\gamma_1} + 2\hat{\gamma_2} n - \hat{\gamma_3} n_{it}^{-2} \tag{1.3}$$

As predicted in the job search model, the probability of exiting unemployment raises as the UI exhaustion point approaches. The model developed in Rothstein (2011) also implied that benefit extensions do not have the same effects on those near exhaustion as on those just beginning their spells. The next specification, from Rothstein (2011), allows that the effect of total benefit

[11] Duration dependence is defined as whether the probability of exiting the state of interest depends upon how long one has stayed in it. A positive(negative) duration dependence refers to a(n) increasing(decreasing) exit hazards as the longer time spent in the state.

weeks Y_{it} for individuals who have been unemployed for less than 26 weeks, defined as short-term unemployed, is different from the effect for individuals who have been unemployed for more than 26 weeks, defined as long-term unemployed. The $Policy_{it}$ becomes:

$$\Pi Policy_{it} = \theta_1 T_{it} I(n_{it} < 26) + \theta_2 T_{it} I(n_{it} \geq 26) + \theta_3 I(n_{it} \geq 26) \tag{1.4}$$

The coefficient θ_1 stands for the effect of T_{it} on the exit hazard contributed by short-term unemployed, and the coefficient θ_2 represents the effect of T_{it} on the exit hazard contributed by long-term unemployed. The effect of T_{it} actually describes the additional rate of exit for people with more benefits. If so, the extension happened to unemployed individuals who just begin their spells should have smaller effect than to unemployed individuals who are close to their exhaustions. In other words, the absolute value of θ_1 should be smaller than the absolute value of θ_2. The coefficient θ_3 captures the pure shift of the fitted log odds at the 26 benefit weeks.

In the two specifications above, the total benefit weeks available to individuals (T_{it}) would have been constant across time for each person if there were no extensions of the UI programs, and hence the changes of T_{it} describes the modification of the police. The next specification from Rothstein (2011) focuses on the interaction between the number of remaining benefit weeks, d_{it}, and the number of benefit weeks that have been used to date, n_{it}. Using the remaining benefit weeks can capture the changes of the exit hazards while moving toward the exhaustion of UI. The $Policy_{it}$ can be written as follow:

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \alpha_2 d_{it} \tag{1.5}$$

Since the remaining benefit weeks decrease one week each week without the extensions of the UI, the variation of d_{it} comes from the addition of new EUC tiers, which extend the potential benefit for individuals who will be able to transit into new tiers before the program expires, but not for individuals who will exhaust their benefit before the new tire implements. Due to the frequent changes of the EUC, d_{it} will not always decrease one week as n_{it} increase one week. In another word, this specification does not produce perfect collinearity while including both d_{it} and used n_{it} at the same time.

Holding constant n_{it}, d_{it} reflects various individuals' expectations of their future benefit.

Higher d_{it} means the unemployed workers are eligible for more extensions, and hence the exit rates would be lower. Rothstein (2011)'s model predicts that when approaching to the UI benefit expiration, search intensity rises, and hence the probability of exit UI for jobs increases as well. Therefore, the sign of the estimated coefficient α_2 is expected to be negative.

In my study, this remaining benefit week actually counts in the evolution of the extension of the EUC. Individual who has higher d_{it} does not necessarily mean that he is at the earlier stage of the UI than another individual who has lower d_{it} and who is supposed to be at the later stage of the UI. Facing the same amount of used benefit weeks, this could be caused by the two individuals enrolled in the UI at different time t, and only the UI benefit of the first individual's got extended, but not of the second individual's.

To better illustrate the relationship between the remaining benefit weeks and the exit hazards, I use the specification which allows unrestricted effect on the remaining benefit weeks d_{it} [12]:

$$\Pi Policy_{it} = \sum_{s=0}^{s=80} \phi_s I(d_{it} = s) \tag{1.6}$$

In this specification, the baseline $B_n(n_{it}; \gamma)$ is a full set of indicators of n_{it}.

The coefficients ϕ capture the unrestricted effect on each week. The solid line in Figure 1.5 illustrates the coefficients of indicators of remaining benefit weeks $\hat{\phi}_s$ versus the corresponding available weeks, and the dashed line represents the lowess-smoothed trend. As indicated in the Figure 1.5, the smoothed line shows a general pattern of $\hat{\phi}_s$ increasing as d_{it} falls toward about 20, then rising rapidly as d_{it} falls further. $\hat{\phi}_s$ tend higher as benefits are exhausted. This implies that the probably of finding employment rises as benefits are exhausted. These are consistent with the general trend predicted from Rothstein (2011)'s model. With depressed search effort, the individuals with many weeks left would have lower exit hazards. As benefit exhaustion approaching, they will increase search effort and reach a maximum value at the time of exhaustion.

Results

Column (1) of Table 1.4 presents logit estimates of first specification of equation (1.1), with standard errors clustered at the individual level. The estimate of Π is -0.00979, which indicates that the higher the total benefit weeks available, the unemployed individuals are less likely to exit into

[12] The maximum value of d_{it} in my data is 80

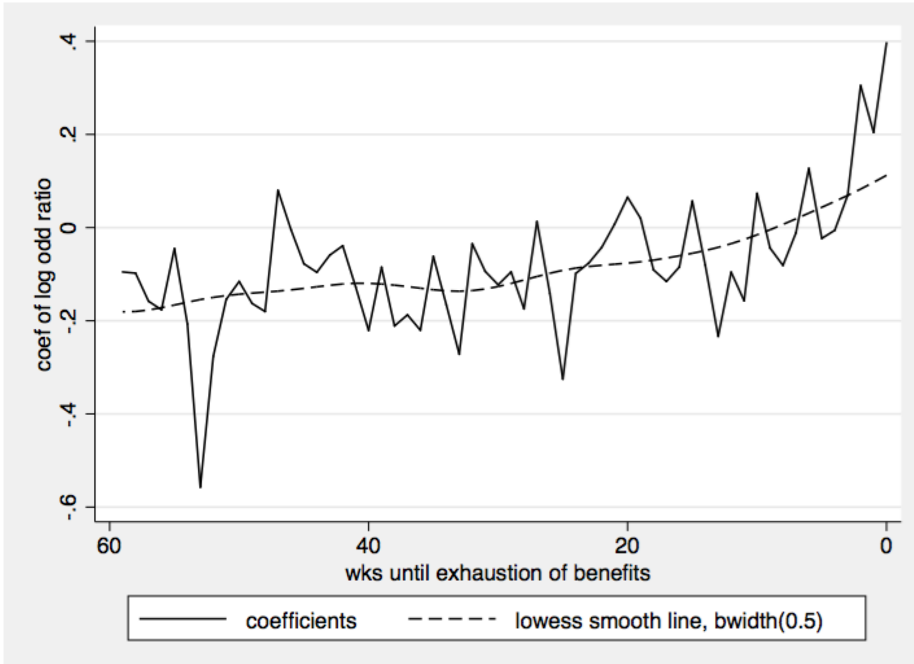


Figure 1.5: Remaining Benefit Effect

employment. The significant negative effect of the number of benefit weeks used, -0.0375, indicates diminishing exit hazards as individuals' unemployment duration increases. Some individuals enrolled and then left UI without drawing any benefit and have 0 benefit weeks recorded. According to the significant positive coefficient on zero-benefit-week indicator, 1.953, these individuals are more likely to exit UI for jobs than people who drew benefit payments.

As indicated in equation (1.3), the baseline duration dependence at n_{it} weeks can be calculated as:

$$\frac{\partial B_n}{\partial n_{it}}|_{n_{it}=n>0} = -0.0375 + 0.000582n + 0.473n_{it}^{-2} \tag{1.7}$$

When n is smaller than 13 weeks, as n becomes lager, the negative duration dependence gets stronger, and the exit hazard decreases at an increasing rate. Between 13 and 64 weeks, the negative duration dependence becomes weaker. At 65 weeks, the baseline effect begins to be positive and increase as n increasing. However, the effect deviated from the baseline is -0.00979 ($\hat{\Pi}$), and this demonstrates that the exit hazard decreases at a constant rate as the unemployed workers stay in UI longer. Since the magnitude of the deviated effect is generally larger than the baseline effect, this first specification predicts that the longer being unemployed, the lower overall exit hazard would be.

Table 1.4: Estimates of Specifications (1.1) and (1.3)

VAR	(1)	(2)	(3)	(4)
	Eqn(1.1)	Eqn(1.1)	Eqn(1.3)	Eqn(1.3)
total # of benefit wks	-0.00979***	-0.00998***		
	(0.0006)	(0.0006)		
total # of benefit wks			0.00263***	0.00247***
(if unemp duration <26)			(0.0006)	(0.0006)
total # of benefit wks			-0.0335***	-0.0336***
(if unemp duration>=26)			(0.0007)	(0.0007)
1(unemp duration=26)			2.897***	2.895***
			(0.0462)	(0.0462)
Controls				
Monthly FE	Y	Y	Y	Y
Demographic info.	Y	Y	Y	Y
Insured unem rate		cubic		cubic
New UI claims rate		cubic		cubic
Base line # of used wks	-0.0375***	-0.0373***	-0.0731***	-0.0730***
	(0.0010)	(0.0010)	(0.0014)	(0.0014)
1(used wks=0)	1.953***	1.961***	2.617***	2.625***
	(0.0656)	(0.0654)	(0.0731)	(0.0730)
(used wks) ^{2}	0.000291***	0.000291***	0.000668***	0.000668***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1/ (used wks)	-0.473***	-0.477***	-0.948***	-0.953***
	(0.0384)	(0.0383)	(0.0438)	(0.0437)
Constant	-2.335***	-2.170***	-2.812***	-2.661***
	(0.1010)	(0.2630)	(0.1050)	(0.2640)
Pseudo-log likelihood	-343504.11	-340644.25	-343451.07	-340624.56
Pseudo R^2	0.0692	0.0693	0.0769	0.0770
Claims	117,504	117,504	117,504	117,504

Column (3) of Table 1.4 shows the results of specification (1.4). For a short-term unemployed individual, the marginal effect of T_{it} on the log odds [13] is $\hat{\theta_1}$ (0.00263). As total benefit weeks increasing, the exit hazards start to climb as well. The coefficient of the indicator, which indicates whether individuals belong to the long-term unemployed group, has a significantly positive estimate, 2.897. This big positive shift captures the changes in the exit hazards between short-term and long-term unemployed workers. People who exhausted the regular 26 benefit weeks and did not continue the extended UI programs contribute the big jump of the coefficient. Besides, people who exhausted the regular UI and got extended to the EUC program have diminishing exit hazard as the total benefit weeks become larger, which is captured by the marginal effect of T_{it} on the long-term unemployment individuals $\hat{\theta_2}$ (-0.0335). Column (2) and (4) of Table 1.4 adds additional controls of slackness to the two specifications respectively: cubics in insured unemployment rates and new UI claims rates. The estimated effects move a bit, but within a very narrow range.

Estimates of this specification (1.5) are shown in the Table 1.5. The first two columns used the same baseline specifications as equation (1.2). To smooth out the week-to-week changes at the beginning of the UI claim durations, and flatten exit spikes brought in by the policies at the end of each tire of UI, column (3) and (4) used splines of the used benefit weeks as a new baseline hazard function [14], and the last two columns used a full set of indicator of n_{it} to represent the baseline hazard.

The first estimate of α_1, as shown in column (1), is -1.984. This negative effect represents that people who still have positive remaining benefit weeks have significantly lower exit hazards than people who are drawing their last benefit. As I discussed above, the estimate of α_2 should also be negative. In my result, the estimates from the first and third baseline specifications are negative, but not the estimates from the second specification. It is also noticeable that the estimates can be changed largely by different forms of baseline hazard functions. Among these three specifications, the full set indicators of the used benefit weeks smoothed spikes of exiting rates at the end of the program better than the others, and hence I will use this baseline hazard function in the following specifications.

[13] Log odds is defined as $\ln(\frac{h_{it}}{1-h_{it}})$

[14] The splines has the form $B_n = \sum_{\nu=0}^{\nu=26} \gamma_\nu n_{it\nu} + \mu_1 I(27 \leq n_{it} \leq 30) + \mu_2 I(31 \leq n_{it} \leq 40) + \mu_3 I(41 \leq n_{it} \leq 50) + \mu_4 I(51 \leq n_{it} \leq 60) + \mu_5 I(61 \leq n_{it} \leq 70) + \mu_6 I(71 \leq n_{it} \leq 80) + \mu_7 I(81 \leq n_{it} \leq 90) + \mu_8 I(91 \leq n_{it} \leq 99)$

Table 1.5: Estimates of Specification (1.5) With Different Controls

VAR	(1)	(2)	(3)	(4)	(5)	(6)
1(remaining benefit>0)	-1.984***	-1.985***	-0.756***	-0.757***	-0.503***	-0.503***
	(0.0281)	(0.0281)	(0.0234)	(0.0234)	(0.0267)	(0.0267)
# remaining benefit wks	-0.00131**	-0.00152**	0.000348	0.000185	-0.00180***	-0.00198***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Controls						
Monthly FE	Y	Y	Y	Y	Y	Y
Demographic info.	Y	Y	Y	Y	Y	Y
Insured unem rate		cubic		cubic		cubic
New UI claims rate		cubic		cubic		cubic
Base line						
# of used wks	-0.0505***	-0.0506***				
	(0.0009)	(0.0009)				
1(used wks=0)	2.162***	2.171***				
	(0.0660)	(0.0659)				
(used wks)^2	0.000300***	0.000300***				
	(0.0000)	(0.0000)				
1/ (used wks)	-0.621***	-0.625***				
	(0.0385)	(0.0384)				
Linear Spline			Y	Y		
Dummies						
for unem durations					Y	Y
Pseudo-log likelihood	-340219.14	-340161.27	-331984.81	-331940.66	-330725.14	-330678.05
Pseudo R^2	0.0781	0.0782	0.1004	0.1005	0.1038	0.1039
Claims	117,504	117,504	117,504	117,504	117,504	117,504

1.5.2 Extensions of the Specifications of Policy Effects

To identify the effect of the policy change on the probability of exiting UI for jobs h_{it}, directly, I define variable Δ_{it} as the change in the number of remaining benefit weeks between week t and week $t-1$ for person i, i.e., $\Delta_{i,t} = d_{i,t} - d_{i,t-1}$, where d_{it} represents the number of remaining benefit weeks. My main specification follows a logistic function:

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \alpha_2 d_{i,t-1} + \delta\Delta_{it} \tag{1.8}$$

In this specification, control M_t^* is a set of fixed time dummies for each calendar month. $B_n(n_{it};\gamma)$ is a full set of indicators of the benefit weeks used, n_{it}, to represent the baseline hazard. Estimating δ will capture the net effect of the policy directly. After trying different functional forms to characterize the baseline hazards (as discussed above), a full set of indicators for the benefit weeks used, n_{it}, best smooths spikes in exit rates at the end of the program.

Moreover, to control the macroeconomic conditions, I include fixed time effects for calendar month, three-order polynomial insured unemployment rates, and three-order polynomial new UI claims rates. Since each policy modification added in (or deducted) almost same numbers of benefit weeks to eligible individuals, the lump of changes happened to claimants at the same time t can be explained by the time dummy. In another word, the change variable is almost collinear with the monthly dummy at time t, if there was a policy change. Since the implementation of UI extensions were exogenous and macro economic conditions prevailing in each month were roughly the same in the nearest month, to fix the collinearity issue, the monthly dummy M_t is substituted by the month M_{t+1} [15] if there was a policy change in month t in this specification and below.

In my main specification, the change in the number of benefit weeks remaining Δ_{it} has three scenarios: 1) There is no change in policy, and claimants consume one week's benefit while moving to the next calendar week, and hence Δ_{it} decreases by one; 2) The number of benefit weeks is increased, so the value of Δ_{it} is larger than -1. 3) During the periods when EUC was paused, the change in the number of benefit weeks was lower than -1.[16] To distinguish between the three

[15] M_{t-1} was also tried, and the results were consistent.

[16] To remove the possible double-negative issue in interpreting coefficients, I use the absolute values of Δ_{it} in my regression. For instance, if d_{it} changed from 4 to 0 at week t, the corresponding Δ_{it}=-4 means the policy actually removed 4 remaining benefit weeks at t. After taking the absolute values of Δ_{it}, $|\Delta_{it}| = 4$ represents the deduction of 4 remaining benefit weeks, and the coefficient measures the effect of one more week being deducted on exit rates.

scenarios, I include three categorical terms in the next specification, which has the form:

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \alpha_2 d_{i,t-1} + \delta_1 \Delta_{it} I(\Delta_{it} > -1) + \delta_2 I(\Delta_{it} = -1) + \delta_3 |\Delta_{it}| I(\Delta_{it} < -1) \tag{1.9}$$

δ_1 estimates the net effect of the policy extension, and δ_2 is the net effect of the policy lapse on unemployment durations.

To better capture the exogenous variations across different individuals who faced the same amount of benefit changes, I interact Δ_{it} with remaining benefit weeks $d_{i,t-1}$, which indicates the stage of UI when the policy changed. Interaction terms like these show that the marginal effect of policy changes can vary with different stages of UI across individuals. The new specifications correspond to the main specification and specification (1.8) can be written as:

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \alpha_2 d_{i,t-1} + \delta \Delta_{it} + \theta d_{i,t-1} * \Delta_{it} \tag{1.10}$$

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \alpha_2 d_{i,t-1} + \delta_1 \Delta_{it} I(\Delta_{it} > -1) + \delta_2 d_{i,t-1} * \Delta_{it} I(\Delta_{it} > -1) + \delta_3 I(\Delta_{it} = -1) + \delta_4 |\Delta_{it}| I(\Delta_{it} < -1) + \delta_5 d_{i,t-1} * |\Delta_{it}| I(\Delta_{it} < -1) \tag{1.11}$$

In my most general specification, I separate the increase in benefit weeks into the different times when the policy was modified. For instance, I included the term $\Delta_{it} I(t_1 \leq \Delta_{it} \leq t_2)$, which represents changes to remaining benefit week which occurred between the first and second policy changes. Benefit changes occurred 12 times during my sample periods, as indicated in Table 1.6, and hence 12 terms are added to my main specification. In the meanwhile, I also control for periods of which extensions the individual experienced before and after a particular extension. For instance, before the second extension, the individual i might have experienced the first extension, and is going to be entitled more benefits in the latter extensions after the second. To identify the effect of the second extensions, I add in dummy controls which exclude the effect from the first extensions, and the extensions in the future. This can guarantee that the coefficient estimates of the term $\Delta_{it} I(t_1 \leq \Delta_{it} \leq t_2)$ isolates the effect of the second extension.

$$\Pi Policy_{it} = \alpha_1 I(d_{it} > 0) + \delta_1 |\Delta_{it}| I(\Delta_{it} < -1) + \delta_2 I(\Delta_{it} = -1) + \sum_{t=1}^{12} \delta_{3t} \Delta_{it} I(\Delta_{it} > -1) I(EP_{it} = t) + \sum_{k=1, k\neq t}^{12} I(P_{ik} = k), \tag{1.12}$$

where EP_{it} represents the extended period which the individual i is experiencing, and P_{ik} controls for the past and future extensions that the individual i have.

Using various specifications, the key set of parameters δ can be identified. I present them in Table 1.6, and will discuss them in details in the next subsection.

Results

The estimates of my main specification (1.8) are shown in the first column of Table 1.6. The estimate of α_1, as shown in column (1), is -0.57. This negative effect represents the fact that individuals who still have benefit weeks remaining have significantly lower exit hazards than people who are drawing their last benefit. The estimate of d_{i-1} is -0.00191, which indicates that the net effect of the UI benefit extension is negative, and the disincentive effect of UI dominates.

After decomposing the policy changes into three scenarios, as shown in Column (3) of Table 1.6, the estimated reduction effect is 0.0148, which describes the positive effect of the reduction in the benefit spells on exit hazards. The more benefit weeks are removed, the higher exit hazards will be. The estimate of δ_2 in equation (1.9) is 0.622. Compared with other individuals who experienced policy changes, unemployed workers whose benefits decrease naturally tend to exit UI for jobs faster. Column (5) of Table 1.6 shows the estimates from my most general specification (1.12). The negative effects of extending UI periods on exit hazards are observed for almost every period.

Column (2) of Table 1.6 shows estimates of equation (1.10). The estimated coefficient of change in remaining benefit weeks ($\hat{\delta}$) is -0.00813, and the estimate of the interaction term ($\hat{\theta}$) is 0.000322. The marginal effect of the change in benefit weeks on the log odds ratio of equation (1.10) can be written as:

$$\frac{\partial ln(\frac{h_{it}}{1-h_{it}})}{\partial \Delta_{it}} = \delta + \theta d_{i,t-1} \tag{1.13}$$

Plugging these two into equation (1.13), the marginal effect of the change on the log odds ratio is $-0.00813{+}0.000322{*}d_{i,t-1}$, and diminishes with the number of benefit weeks remaining. When $d_{i,t-1}$

Table 1.6: Effects of UI period on Unemployment spells

Variables	(1)	(2)	(3)	(4)
	Eqn(1.8)	Eqn(1.10)	Eqn(1.9)	Eqn(1.11)
1(remaining benefit>0)	-0.570***	-0.568***	-0.747***	-0.747***
	(0.0282)	(0.0282)	(0.0302)	(0.0302)
# remaining benefit wks at t-1	-0.00191***	-0.00181***	-0.00144**	-0.00187***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
# of remaining wks changed	0.00136	-0.00813***		
	(0.0013)	(0.0029)		
(#wks remainins at t-1) *(# wks changed)		0.000322***		
		(0.0001)		
Extension Effect			0.0166***	-0.00508*
			(0.0018)	(0.00287)
(#wks remainins at t-1) *(# wks increased)				0.000480***
				(0.0001)
Reduction Effect			0.0148***	0.0545***
			(0.0026)	(0.0107)
(#wks remainins at t-1) *(# wks reduced)				-0.00108***
				(0.0003)
I(remaining wks reduced naturally)			0.622***	0.676***
			(0.0416)	(0.0437)
Pseudo-log likelihood	-276980.26	-276967.44	-276860.55	-276831.11
Pseudo R^2	0.1021	0.1108	0.1210	0.1500
Claims	117,504	117,504	117,504	117,504

Note: In these five specifications, I control for the individual-level covariates, fixed time effects per calendar month, three-order polynomial weekly insured unemployment rates, three-order polynomial weekly new UI claims rates, and baseline hazard. Robust standard errors in parentheses. ***(**) (*) indicates statistical significance at the 1% (5%) (10%) level.

Variables	(5)
	Eqn(1.12)
1(remaining benefit>0)	-0.639***
	(0.136)
# of remaining wks reduced by policy	0.421*
	(-0.25)
I(remaining wks reduced naturally)	0.246***
	(0.0895)
# of increased benefit wks after 1st policy change	-0.0325**
	(0.0129)
# of increased benefit wks after 2nd policy change	-0.0143***
	(0.0040)
# of increased benefit wks after 3rd policy change	-0.0080
	(0.0115)
# of increased benefit wks after 4th policy change	-0.0400*
	(0.0211)
# of increased benefit wks after 5th policy change	-0.0510***
	(0.0167)
# of increased benefit wks after 6th policy change	-0.0210*
	(0.0112)
# of increased benefit wks after 7th policy change	-0.0496***
	(0.0188)
# of increased benefit wks after 8th policy change	-0.0173*
	(0.0097)
# of increased benefit wks after 9th policy change	-0.0113***
	(0.0035)
# of increased benefit wks after 10th policy change	0.0108*
	(0.0060)
# of increased benefit wks after 11th policy change	0.0126
	(0.0127)
# of increased benefit wks after 12th policy change	0.0022
	(0.0094)
Pseudo-log likelihood	-31979.608
Pseudo R^2	0.2178
Claims	117,504

is smaller and equal to 25 weeks, the marginal effect is negative, which implies that the disincentive effect of UI dominates and becomes more pronounced as approaching to the exhaustion. Conversely, the positive effect of UI on unemployment duration dominates when more than 26 benefit weeks remain, and moving towards the beginning of the spell, the number of extra benefit weeks added could accelerate the rates of exit for employment.

Results from the similar interaction strategy in equation (1.11) are presented in Column (4) of Table 1.6. The estimate of the interaction terms for the benefit reduction is -0.00108. The negative estimate explains that the marginal effect of the reduction in benefit durations on the log odds increases as fewer benefit weeks remain, which implies that reductions from policy change occurring at the end of the UI spell (with smaller values of $d_{i,t-1}$) increase claimants' job finding rates more than if the reduction occurs at the beginning of the spell.

1.5.3 Simulate the effects of EUC Policy

The results in previous tables indicate that extensions to UI periods reduced the probability that a UI recipient found a job. To quantify the estimate in terms of interpreting the extension effect, this section presents simulations of the net effect of the extensions on unemployment spells, which is obtained by comparing actual unemployment exit hazards with counterfactual hazards that would have existed without extensions. In the logit model, the predicted average weekly hazard from prior specifications is calculated as:

$$\hat{h}(V_t, t) = \frac{1}{1 + e^{-\hat{\beta}'(V_t + B_n(n_{it};\gamma) + M_t^* + X_i)}} \tag{1.14}$$

where $\hat{\beta}'$ is the estimated coefficients from prior specifications. V_t is the policy variables, which include d_{it} and Δ_{it}, and measure the policy changes over time and across individuals. In this part, I reconstruct the policy variables by assuming there is no extension for each unemployed individual, and the maximum benefit weeks they could claim is 26 weeks. The predicted counterfactual weekly hazard is calculated by using new values for these reconstructed variables and estimated from the corresponding specifications:

$$\hat{h}_{CF}(V_{CF,t}, t) = \frac{1}{1 + e^{-\hat{\beta}'(V_{CF,t} + B_n(n_{it};\gamma) + M_t^* + X_i)}} \tag{1.15}$$

where $V_{CF,t}$ are reconstructed policy variables with counterfactual values.

Figure 1.6 plot the actual, counterfactual, and 95% weekly hazards predicted by different specifications. The shaded areas are the simulated confidence intervals for the counterfactual hazard at the 95% confidence interval. In general, the counterfactual hazards are higher than the actual ones, which supports the prediction that reducing the number of benefit weeks would increase the probability of exiting UI for employment. Another notable pattern is spikes in the hazard just before each program reaches exhaustion. The spike in the exit rates from unemployment at the expiration of jobless benefits is documented as one of the best-known empirical results in labor economics. It is widely interpreted as evidence that recipients wait until their benefits run out to search intensively and return to work.

These figures also give a closer examinations of the baseline hazard functions. Figure 1.6.a and 1.6.b use estimates from equation (1.1) and (1.3), respectively, but with the same baseline functions as indicated in Table 1.4 Column (2) and (4). However, the two shapes of hazards are distinct from each other, even for the weeks before 26, which shouldn't have changed much in both actual and counterfactual scenarios. It seems the baseline hazard function fails to capture the variation brought in by the sample, and induces different specifications to produce non-robust results. Figure 1.6c and Figure 1.6d adopts same specification of equation (1.5) but different baseline functions from Table 1.5 Column(2) and (6). It is obvious to see that the one used to plot 6.b, the full set indicators of used benefit weeks, generates much smaller standard errors of the counterfactual hazards, and captures the spikes of exiting around the exhaustion of UI better. Applying this baseline function, I get consistent shapes of the predicted hazards from different specifications used in Figure 3.3e and 3.3f which uses equation (1.8) and (1.9).

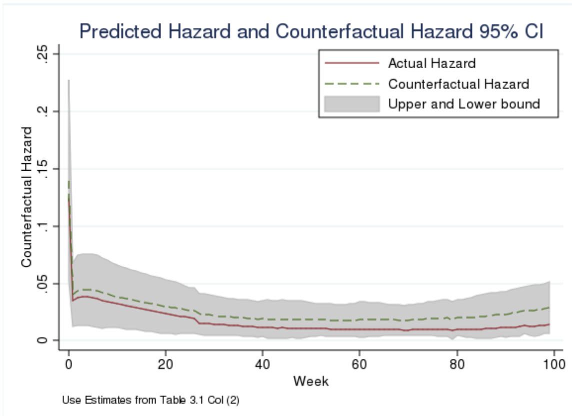
The difference between the actual average weekly hazards and the counterfactual average weekly hazards, denoted as Δh_t, captures the effects of the UI extensions on the average exit hazards. Let I_n represent the size of each sample with a different number of benefit weeks used, n, then:

$$\Delta h_t = \overline{\hat{h}(t)} - \overline{\hat{h}_{cf}(t)} = \frac{1}{I_n} \sum_{i \epsilon n_t} \left(\frac{1}{1 + e^{-\hat{\beta}'(V_t + B_n(n_{it};\gamma) + M_t^* + X_i)}} - \frac{1}{1 + e^{-\hat{\beta}'(V_{cf,t} + B_n(n_{it};\gamma) + M_t^* + X_i)}} \right) \tag{1.16}$$

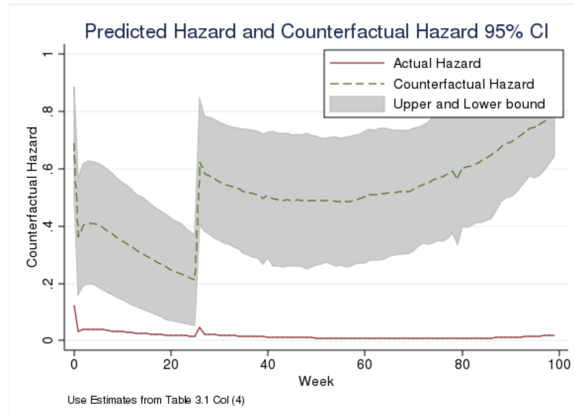
The first column in Table 1.7 shows the actual average monthly hazards, which are converted from the corresponding weekly hazards calculated from equation (1.14). Row Table 1.4 Column (2) shows the

Figure 1.6: Predicted Hazard and Counterfactual Hazard

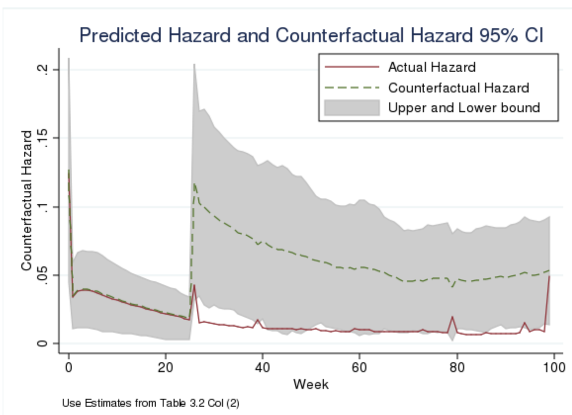
(a)



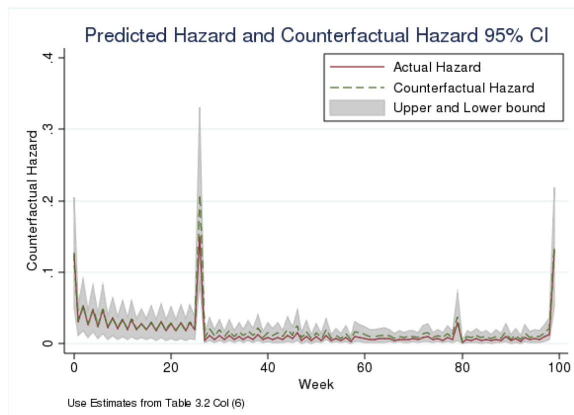
(b)



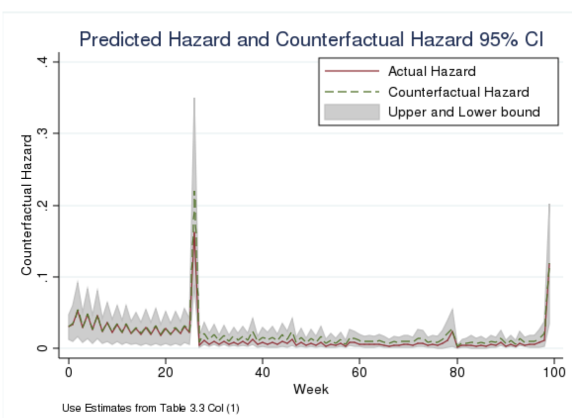
(c)



(d)



(e)



(f)

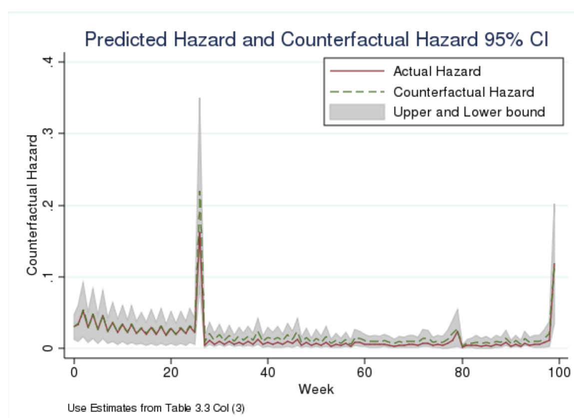


Table 1.7: Hazard rates of exiting UI for jobs

	Actual Avg. Mon Exit Hazard	C.F Avg. Mon Exit Hazard	$\hat{h}_{Actual} - \hat{h}_{CF}$	$E(D) - E_{CF}(D)$	Δ # wk of D given one more benefit wk
Table 1.4	0.1042	0.1105	-0.0063	28.4930	0.3903
Column (2)	(0.0062)	(0.0076)			
Table 1.4	0.1050	0.1119	-0.0069	29.3039	0.4014
Column (4)	(0.0063)	(0.0077)			
Table 1.5	0.1058	0.1100	-0.0042	28.6574	0.3926
Column (2)	(0.0089)	(0.0213)			
Table 1.5	0.1066	0.1113	-0.0047	29.0080	0.3974
Column (6)	(0.0064)	(0.0078)			
Table 1.6	0.1065	0.1189	-0.0123	31.2002	0.4274
Column (1)	(0.0210)	(0.0470)			
Table 1.6	0.1085	0.1193	-0.0108	31.3203	0.4126
Column (3)	(0.0063)	(0.0077)			

Note: To better capture the extension effect, in the simulation part, I use the sample which only experienced the UI extension, but not before EUC enacted or EUC paused. Each row name represents Table number and Column number. Simulated standard errors are given in parentheses.

predicted hazards from estimates of my specifications in Table 1.4 Column 2. Overall, the predicted monthly hazards in the fourth quarter of 2010 are broadly consistent across different specifications. The same pattern is also observed in the second column, counterfactual predicted monthly hazard. The third column of Table 1.7 is the difference between actual hazards and counterfactual hazards, which indicates the changes to the exit probability by extending UI from 26 weeks to 99 weeks. In general, implementing the UI extension decreases exit hazards for jobs by 0.007 to 0.01. [17]

I also simulate the effect of changes in the length of UI benefits on the duration of unemployment spells, using the methodology of Farber and Valletta (2013). I assume that individuals' behavior is consistent after benefits are exhausted, and hence the weekly hazard of a spell ending after the maximum benefit duration is constant as well. The constant feature implies the conditional distribution of hazard is exponential and the expected unemployment spells for individuals censored

[17] Rothstein (2011) concludes that extending UI decreases the rate of exiting for jobs by 0.005 (0.5 percentage point) for workers who are eligible for unemployed insurance.

from the exhaustion is the inverse of the constant hazard.

The key quantity used in the simulation is the predicted survivor function for each individual i in week t. The predicted survivor function in week t is the predicted probability that a UI spell lasts until at least t:

$$\hat{S}_i(t) = (1 - \hat{h_i}(t)) \prod_{\tau=1}^{t-1} S_i(\tau) = \prod_{\tau=1}^{t} (1 - \hat{h_i}(\tau)), \tag{1.17}$$

where $\hat{h_i}(\tau)$ is the estimated unemployment exit probability for individual i in week τ. I further assume that the weekly hazard of a spell ending after 99 weeks is constant for each individual at the average value for that individual of the hazard from weeks 79 to 99. On this basis, the expected duration of each spell is,

$$E_i(D) = [\sum_{\tau=1}^{99} \tau \hat{h}_i(\tau) \hat{S}_i(\tau - 1)] + \hat{S}(99) \frac{1}{\bar{h}_i}, \tag{1.18}$$

where $\bar{h_i}$ is the average hazard across weeks 79-99 of the estimated weekly hazard of the unemployment spell of individual i. In my counterfactual study, the maximum number of benefit weeks is 26; therefore, the counterfactual expected unemployment duration is calculated as a rolling sum of 26 weeks.

Similarly, I use the predicted hazard from the counterfactual study to simulate $\bar{S}(t)_{cf}$ and $E_{cf}(D)$ assuming a maximum of 26 benefit weeks. The difference between $E(D)$ and $E_{cf}(D)$ is the change in unemployment spells given the extension of UI, which are shown in the fourth column of Table 3.5. Adding 79 benefit weeks during the Great Recession increased the average unemployment spell from 28 to 31 weeks, and hence the average unemployment spell increased 0.4 weeks with the addition of an extra benefit week, as shown in the last column of Table 3.5. This finding is larger than Farber and Valletta (2013)'s result, which suggests that extended benefit increased unemployment spells by about 7% (1.96 weeks on the basis of 28 weeks). On the other hand, my estimates are consistent with previous estimates in the US using administrative data, such as Card and Levine (2000), Katz and Meyer (1990), and Landais (2013).[18]

Generally, my estimates are higher than the recent empirical studies which use CPS data (Rothstein (2011), Farber and Valletta (2013) and Fujita (2011)). CPS data arguably identify the

[18]These papers' estimates suggest that the duration of UI claims increase by around 0.3 to 0.4 weeks on average by increasing the potential duration of benefit by a week. These studies define the dependent variable as the probability of exhausting all available benefit while leaving UI, instead of the probability of finding jobs. To make the comparison meaningful, I redefine my dependent variable in the same way, and simulate hazard rates of exhausting UI and the corresponding expected durations of UI claims.

eligibility of UI recipients, and put strong assumptions on the exogenous policy variations across individuals. My detailed administrative data help me use flexible specifications to more accurately capture the variations in UI across time and individuals. My results also suggest that, during the Great Recession, the supply response of unemployment to the multiple extensions was not extremely different from what previous studies suggest. The disincentive effect of the UI still dominated and affected people's search effort almost the same as before.

1.5.4 Difference-in-Differences Estimates

To identify the effects of a change in UI benefit weeks on the possibilities of leaving UI for jobs, I can also compare the pattern of UI recipients before and after the policy change. The economic shocks could affect job search results for both the group affected by the law modification (treatment group) and the group not qualified for the extra benefit from the new law (control group). In the meanwhile, the new policy could influence the incentive and search effort only those who are eligible for the change. Since I am interested in identifying the treatment effect, the effect of the systematic shocks to the labor market should be controlled.

Conventionally, this can be solved by a difference-in-differences (DD) strategy in which the model takes the difference in the behavior before and after the new policy for a treatment group and subtract from the difference in the behavior before and after the new policy of a group that is not affected by the new change. The change of the behavior in the control group can capture the general economic shocks and labor market trend.

In my study, individuals whose UI spells were in progress after the announcement of a UI extension, yet ineligible for the extension may nonetheless have been affected by the policy – that is, by the very announcement itself. A difference-in-difference estimation strategy allows me to account for this possibility. I start by separating the group of UI claims that are eligible for each EUC extension from the claims which are not qualified for the corresponding extension before and after the new policy announced. My base comparison is the difference between the averages for the extension eligible and ineligible individuals in the treatment group.

Denote $EUC_{ijt} = 1$ if individual i was eligible for the j^{th} extension at time t, and 0 otherwise. Denote, too, the indicator $A_{ijt} = 1$ if the individual's spell of unemployment falls before the announcement of the j^{th} policy change and 0 otherwise. To account for the fact that the filing dates can be possibly correlated with the unobserved factors which could affect the exit hazards, I also control for the filing dates of UI claims, thereby controlling for the comparison is among claims which filed at similar macro circumstances. The dependent variable is the same as the previous specifications, Y_{ijt}, which is one if individual i leaving UI for a job, and zero otherwise. Using the same logit model, the DD specification has the following form:

$$ln(\frac{h_{ijt}}{1-h_{ijt}}) = \alpha I(EUC_{ijt}=1) + \beta I(A_{ijt}=1) + \delta I(EUC_{ijt}=1) I(A_{ijt}=1) + \gamma n_{it} + X_{ijt} + M_{ijt} + \varepsilon_{ijt}, \tag{1.19}$$

where h_{ijt} is the probability of exiting UI for employment for individual i in time period t for the j^{th} extension (*i.e.* $h_{ijt} = Prob(Y_{ijt}) = 1$). n_{it} controls the used benefit weeks for person i in period t, X_i is a vector of other controls (such as age, gender and years of schooling), M_{it} is a set of indicators for calendar months to control the time effect and ε_{it} is the unobserved error term.

Relative to an individual in a UI spell prior to the announcement of the j^{th} policy change (see Table 1.2), and prior to the start date of EUC eligibility, the effect of being within the relevant policy window equals P(EUC_{ijt}=1, A_{ijt}=0)=α. The relative effect of being in a UI spell $after$ the announcement of the policy change, and $after$ the date of the EUC eligibility window is equal to P(EUC_{ijt}=0, A_{ijt}=1)=β, and the relative effect of being in a spell after the announcement date and within the policy window is equal to P(EUC_{ijt}=0, A_{ijt}=1)=α+β+γ.

The coefficient on the interaction term is the difference-in-differences estimates. The key extension effect on h_{ijt} can be identified by:

$$\begin{aligned}\delta \equiv &(E[ln(\frac{h_{ijt}}{1-h_{ijt}})|EUC_{i,j,t+1}=1, A_{i,j,t+1}=1] - E[ln(\frac{h_{ijt}}{1-h_{ijt}})|EUC_{i,j,t}=1, A_{i,j,t}=0]) \\ &- (ln(\frac{h_{kjt}}{1-h_{kjt}})|EUC_{k,j,t+1}=0, A_{k,j,t+1}=1] - E[ln(\frac{h_{kjt}}{1-h_{kjt}})|EUC_{k,j,t}=0, A_{k,j,t}=0])\end{aligned} \tag{1.20}$$

If the disincentive effect of the extension dominates, then the $\hat{\delta}$ is expected to be negative.

There are 11 EUC extensions during my sampled period, and hence I apply this specification in 11 different extension periods. The results are presented in Table 1.8. The coefficient estimates $\hat{\alpha}$ are almost negative and significant for every policy change, which indicates that on average the group of individuals who are eligible for extensions has lower exiting rates. Among those who are not eligible for the extensions, the pattern of changes in the probabilities of exiting rates for employment before and after the new policy announced is not as obvious as the one in the treatment group. In the latter extension periods, from euc_5 through euc_{10}, the estimates of the β are also significant and negative for the control group. These changes can be due to the effects from the labor market, instead the extended unemployment insurance.

Table 1.8: Difference-in-Difference Estimates

	Emergency Unemployment Compensation Policy Period										
VARIABLES	1	2	3	4	5	6	7	8	9	10	11
I(Eligible for EUC_j=1)	-0.268***	-0.213***	-0.281***	0.146***	-0.00258	-0.0702***	0.0093	-0.173***	-0.210***	-0.165***	-0.0824***
	(0.035)	(0.027)	-0.0383	-0.0209	-0.015	-0.0159	-0.0139	-0.0219	-0.0255	-0.0168	-0.0224
I(Announced=1)	0.0485	-0.0399	-0.587***	-0.0817	-0.151**	-0.330***	-0.241***	-0.485***	-0.436***	-0.203**	0.245
	(0.108)	(0.065)	(0.074)	(0.069)	(0.067)	(0.103)	(0.055)	(0.070)	(0.081)	(0.082)	(0.165)
DD estimates	-0.130***	-0.130***	-0.489***	-0.765***	-0.469***	-0.605***	-0.683***	-0.779***	-0.365***	-0.625***	0.289**
	(0.048)	(0.045)	(0.046)	(0.071)	(0.034)	(0.045)	(0.038)	(0.037)	(0.042)	(0.046)	(0.124)
Used benefit wks	-0.0257***	-0.0257***	-0.0264***	-0.0263***	-0.0267***	-0.0264***	-0.0270***	-0.0283***	-0.0267***	-0.0264***	-0.0261***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-2.649***	-2.564***	-1.984***	-2.492***	-2.398***	-2.567***	-2.297***	-1.987***	-2.139***	-2.493***	-2.489***
	(0.135)	(0.172)	(0.176)	(0.173)	(0.173)	(0.159)	(0.168)	(0.173)	(0.178)	(0.177)	(0.193)
Claims	117,504	117,504	117,504	117,504	117,504	117,504	117,504	117,504	117,504	117,504	117,504

Note: See Table 1.2 for a list of the policy periods.

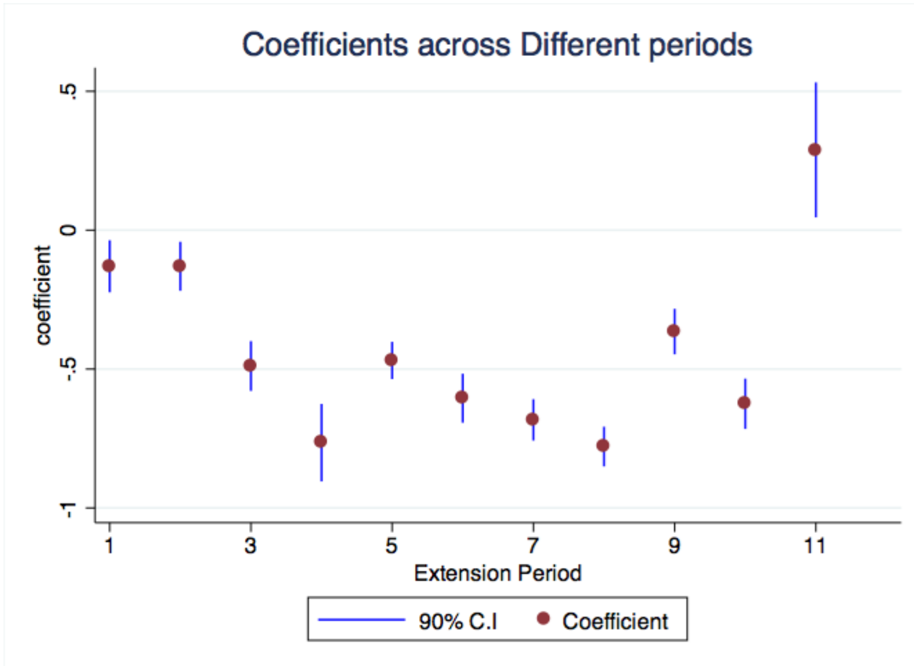


Figure 1.7: Estimates across Different Extension Periods

The diff-in-diff estimates $\hat{\delta}$ are negative in 10 out of 11 extension periods, and this key estimator provides evidence that the disincentive effect of the UI extension dominates in almost each period. Figure 1.7 shows the DD estimates in various extension periods. The pattern implies that the magnitude increases at the beginning and then decreases as the program extended repeatedly, which suggests that UI recipients do not respond to the extensions monotonically.

1.6 Conclusion

In this paper, I use administrative data from Kentucky to examine two dimensions of the effect of the extension of the UI period. My estimates from logit models and other alternative hazard models indicate that the net effect of extended benefit coverage on unemployment spells is negative, and hence the disincentive of UI extension dominates at most levels of remaining benefit weeks. I find that implementing the UI extension decreased exit hazards for jobs by 0.01, which is higher than the existing studies for the multiple extensions UI but using CPS data. My detailed administrative data and flexible specifications help me capture the policy variations across individuals and over time, without putting strong assumptions on the eligibility of the UI recipients, as other studies using CPS do. My results suggest that during the Great Recession the supply response of unemployment to the multiple extensions was not extremely different: the disincentive effect of the UI still dominated and affected people's search efforts almost as much as before.

Chapter 2

The Effect of UI Benefit Level Changes on the Duration of Unemployment

2.1 Introduction

Over the course of the Great Recession, the national unemployment rate rose from 6% in the middle of 2008 to almost 10% in the latter months of 2009. Although the recession ended officially in June 2009, unemployment remained stubbornly high, and the unemployment rate did not fall below 9% until January 2012, over two years after the Great Recession ended. Even more strikingly, the long-term unemployment rate[1] reached much higher levels (45.5%, the historic high) which persisted much longer in the Great Recession than in any previous period since the late 1940s. While the unemployment rate slowly receded after the peak of about 12%, the long-term unemployment rate remained staggeringly high at more than 39%[2], and the mean durations of unemployment reached 40 weeks (historic high) at June 2010, one year after the Great Recession ended.

In addition to the extension of UI periods, weekly benefit levels also increased in Kentucky over the course of the recession. Unlike most states in the US, the weekly benefit payout eligibility

[1]The long-term unemployment rate is defined as the share of the unemployed who have been out of work for 27 weeks (six months) or more.

[2]The highest level in the history was 26%

in Kentucky is independent of the number of weeks of benefit eligibility. This setting allows me to distinguish between UI benefit level and the number of weeks of UI entitlement in terms of their effects on unemployment durations.

Since the UI benefit level is determined by eligible unemployed workers' prior earnings, conventional identification strategies usually encounter endogeneity issues. As has been noted in recent studies (Nielsen et al. (2010)[3] and Simonsen et al. (2010)), using the kinks in the benefit policy provides the possibility for identifying the effect of the policy variable, while overcoming the traditional endogeneity issue, even in the absence of traditional instruments. I identify the effect of UI weekly benefit level by adopting the Regression Kink Design (RKD), formally developed by Card et al. (2012), using the kinks in the schedule of UI benefits. My estimates suggest that the elasticity of unemployment duration with respect to the benefit level is in the range of 0.2 to 0.5, and hence the increase of maximum UI benefit level (from $401 to $440) raises the unemployment duration for the median person[4] by 0.33 to 0.83 weeks. Moreover, my estimates indicate that the effect of unemployment insurance benefits could vary over the lifecycle, and the duration elasticity is smaller when the unemployment rate is relatively high.

2.2 Studies of the effect of Unemployment Benefit on the Duration of Unemployment in the U.S.

Among the studies which investigate the effect of benefit level on unemployment duration, it is typical to regress the durations on the benefit level. However, the weekly benefit is a constant fraction of previous earnings (when the qualified benefit level is smaller than the cap), and hence such regressions cannot distinguish between the effect of UI and the highly correlated influence of previous earnings. In addition, other sources of differences in benefits, such as family composition and allowances for dependents, are also likely to have dependent effects on unemployment spells. To overcome this traditional endogeneity issue, a growing literature considers identifying models with endogenous regressors conditional on a suitable control function. But this identification hinges on the existence of one or more "instrument" variables that are assumed to be independent of

[3]The Regression Kink Design (RKD) was first introduced by Nielsen et al. (2010) to identify the causal effect of financial aid on college enrollment.

[4]Using the median of unemployment durations in the US at the beginning of the Great Recession, which was 17 weeks

the errors in the regression functions. Unfortunately, it is difficult to find appropriate instruments that satisfy valid assumptions particularly when the regressor of interest is a policy variable. The level of unemployment benefits, for example, is typically set by a formula that depends on previous earnings. In such settings individual characteristics that affect the benefit level could also correlated with any unobserved determinants of previous earnings, and hence are correlated with unemployment duration.

Another approach to identifying the benefit effect uses exogenous variation from policy changes: for example, the sharp discontinuities in the potential duration of benefit entitlements by age. Such identification of a causal effect (Regression Discontinuity Design) relies on a discrete change or a jump in the treatment probability, and yet fails or is weak when there is no discontinuity, or when the discontinuity is small (Lee and Lemieux (2010)). In the context of the US benefit schedule, no such policy discontinuity exists; instead, the unemployment benefit level is determined by a formula which is a fixed fraction of prior-job earnings and subject to a maximum amount.

As has been noted in recent studies (Nielsen et al. (2010)[5] and Simonsen et al. (2010)), the existence of a kinked policy rule provides the possibility for identifying the effect of the policy variable, even in the absence of traditional instruments. Essentially, the Regression Kink Design (RKD) looks for an induced kink in the outcome variable that coincides with the kink in the policy rule and relates the relative magnitudes of the two kinks. By applying RKD, one can still identify a policy effect even if there is no discontinuity in the policy, provided there is a kink, or a slope change in the assignment variable in the continuous treatment.

Card et al. (2012) formally considers nonparametric identification of the average marginal effect of a continuous endogenous treatment variable in a generalized nonseparable model, where the treatment of interest is a continuous but kinked function of an observed assignment variable. A valid RKD is achieved by assuming that, conditional on the unobservable determinants of the outcome variable, the density of the assignment variable is smooth at the kink point in the policy rule (established in Card et al. (2012)). This crucial assumption rules out the possible endogenous issue that the agents can deterministically manipulate the value of the assignment variables in the policy formula. For example, a kink in the marginal tax schedule would be expected to lead to taxpayers bunching up at the level of income associated with the kink. Such an endogenous issue could lead

[5]The Regression Kink Design (RKD) was first introduced by Nielsen et al. (2010) to identify the causal effect of financial aid on college enrollment.

to a non-smooth distribution of unobserved heterogeneity around the kink point. Therefore, under the smooth density condition, the endogenous chosen concern can be solved. Following the RD design (Lee and Lemieux (2010)), RKD validity is testable. In contrast to the traditional approach, RKD predicts that predetermined characteristics are irrelevant and unnecessary for identification, and hence RKD can avoid making any strong assumptions about which characteristics should be included in the analysis.

2.3 The Unemployment Insurance System In Kentucky

UI benefit is based on a formula that depends on the claimant's labor market activities in the period before becoming unemployed. This period, defined as the base period, is traditionally the first four completed calendar quarters out of five immediately preceding the start of the claim.[6] The weekly benefit rate payable to a compensated unemployed person is an amount equal to 1.3078% of his earnings in the base period, provided this is not more than the maximum amount.

During my study period, benefit level changed three time. The maximum weekly benefit rose from \$401 in 2006 to \$415 in 2007, and all claims filed after December 21, 2008 received extra \$25 every week. Hence, claimants who had not exhausted their UI benefit before the end of 2008 would have more benefit before exhaustion than the amount determined at the start of the claim. Let b denote "weekly benefit amount" and let E_{1-4} is the sum of the quarterly earnings in the base period. The rules that determine b are:

$$b_{2006-2007} = \begin{cases} 1.3078\% * (E_{1-4}), & \text{if } E_{1-4} \leq \$30662.18. \\ \$401, & \text{if } E_{1-4} > \$30662.18. \end{cases}$$

$$b_{2007-2009} = \begin{cases} 1.3078\% * (E_{1-4}), & \text{if } E_{1-4} \leq \$31732.68. \\ \$415, & \text{if } E_{1-4} > \$31732.68. \end{cases}$$

$$b_{2009-2011} = \begin{cases} 1.3078\% * (E_{1-4}) + \$25, & \text{if } E_{1-4} \leq \$31732.68. \\ \$440, & \text{if } E_{1-4} > \$31732.68. \end{cases}$$

[6] During the sampled period, Kentucky was one of a dozen states that had not adopted the Alternative Base Period, which is the most recent four completed quarters prior to unemployment. In most states, individuals who did not qualify under the conventional method of determining eligibility could be reconsidered using the Alternative Base Period.

The amount of weekly benefit does not change once the claim starts, even if the claim is extended into EUC or EB. Figure 2.1 shows the rule for determining the weekly benefit amount as a function of base-period earnings. The bottom line corresponds to the rule before 2007, and the middle line is the rule after 2007 and before 2009. The replacement rates are not changed before the threshold for the whole sampled period.

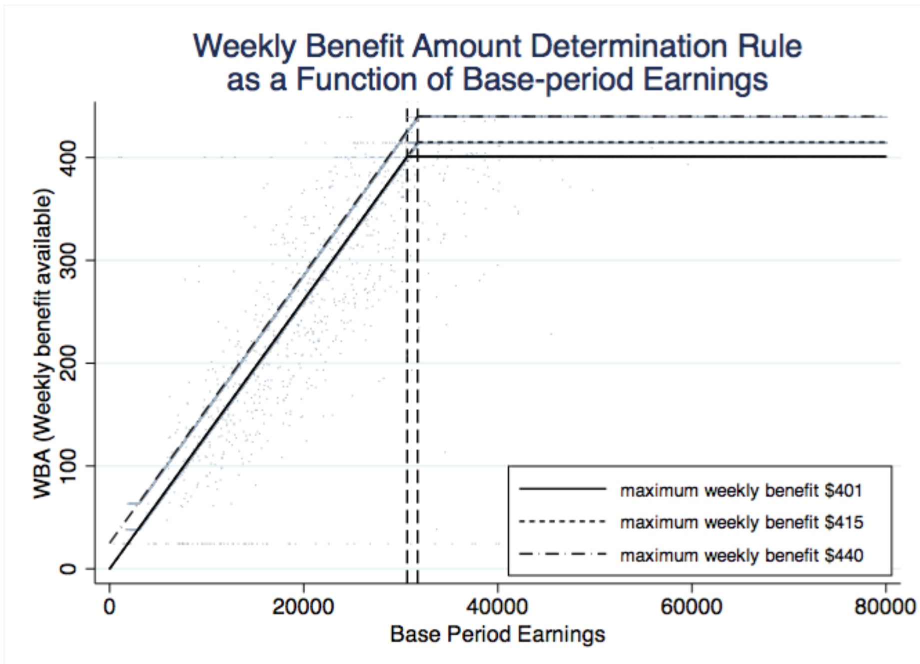
In my study, the weekly benefit amount is not a function of the number of weeks claimed. In contrast, the conventional setting usually involves in two endogenous regressors (weekly benefit amount and the number of benefit weeks), which are kinked at the same point. But this setting makes it difficult to distinguish between the independent effects of benefits on unemployment duration and the effects of the number of weeks of unemployment benefit entitlement on unemployment duration. Hence, the study in Kentucky would plausibly give a more accurate estimate of the effects of the weekly benefit amount and benefit durations on the unemployment spells.

2.4 Data

In this paper, I use data drawn from the Kentucky Office of Employment and Training [7]. The administrative data contain quarterly records for each individual filing for UI compensation from January 2006 through December 2011. The information relevant for my study includes the number of weeks for each claim; claimants drew benefit from which UI program; the benefit amount for which each claimant was eligible; the actual amount of benefit they drew; base-period earnings, and demographic characteristics such as age, gender, ethnicity, employer NAICS code, and years of schooling.

In addition, I have another important resource for this study is the quarterly wage files, which cover UI recipients in my sample from 2005 through 2013. The wage file contains detailed information about these UI claimants before and after they were in the UI programs, including earnings both before and after their UI spell and major industry sectors. Unlike most of the administrative data used in previous research, merging the data file helps me to track individuals into their first post-UI employment spell and permits me to calculate individuals' unemployment durations and examine the effects of UI not simply on the probability of exiting UI, but on the probability of finding employment.

[7] The data were randomized and anonymous without personal identifiers.



Notes: The bottom line corresponds to the rule before 2007, and the middle line is the rule for 2007-2008. The top line represents the rule after December 21, 2008. Base-period earnings are the sum of earnings over the first four calendar quarters out of five immediately preceding the start of the claim. Comparing the bottom and the middle line, the maximum weekly amount increases by $14 and the minimum base-period earnings that qualify for the maximum amount shifts from $30,662.18 to $31,732.68, as indicated by the two vertical dashed lines. The top line shifts up by $25 from the middle line, and represents the extra $25 added into UI weekly benefit for every claimant. The threshold of base earnings is still $31,732.68, and the replacement rates (the slope of the function before the cutoff) are not changed for the whole sampled period. The figure only includes observations with base-period earnings less than $80,000.

Figure 2.1: UI Benefit Schedule

Table 1.3 reports summary statistics of variables for the analyzed sample. The sample includes 71879 displaced workers who files 95365 UI claims.[8] Around 13.3% ($=\frac{12676}{95365}$) of claims were filed before EUC was authorized and were not eligible for the extension. Base-period earnings increases to $32,476.32 in the group which filed claims after EUC enacted, and yet below the wage cutoff eligible for the benefit cap.

2.5 Theoretical Model

Conventional search models (Mortensen (1977)) suggest that a higher UI benefits level reduces incentives for job search, leading to an increase in the expected duration of joblessness. According to the policy formula, the benefit level is proportionate to the wage of the prior job w_{-1}, up to some maximum benefit level B_{max}. To better capture the benefit effect of the kinked policy, I develop upon the model of Rothstein (2011), which uses a simplified dynamic job search model assuming that the duration of unemployment spells is solely determined by unemployed workers' job search efforts s in each period, by including the UI benefit level b as one of the state variables.

Following the set up in Rothstein (2011), d represents the remaining benefit weeks. $p(s)$ is the probability that a claimant finds a job in a period given search effort s, which has $p'(s) > 0$ and $p''(s) < 0$. The Bellman equation becomes:

$$V_U(b,d) = \max_{s_{b,d}} u(b) - s_{b,d} + \delta[p(s_{b,d})V_E(w_{-1},b) + (1-p(s_{b,d}))V_U(b,d-1)], \text{ where } b < B_{max} \tag{2.1}$$

The first order condition of equation (1) then implies that the search effort choice satisfies:

$$p'(s_{b,d}) = \frac{1}{\delta(V_E(w_{-1},b) - V_U(b,d-1))}, \text{ where } b < B_{max} \tag{2.2}$$

As proved in Rothstein (2011), the value of unemployment, i.e., $V_u(b,d)$, increases with the remaining benefit weeks d, and decreases with search effort s. It is easy to show that $V_u(b,d)$ increases with b, and hence with the level of the previous wage w_{-1}, which determines the benefit level, up to the threshold for the maximum benefit. On the other hand, higher-wage jobs entitle individuals

[8] In analyzing the effect of extending UI periods, I use all observations in my sample. However in my analysis of the effect of UI benefits, I exclude observations with censored unemployment durations and wrongly recorded base-period earnings.

Table 2.1: Summary Statistics II

Sample for the study into the effect of UI benefit levels

	2006-2007	2007 and After
# of Claims	12,676	82,598
Base-period earnings	\$29,231.19	\$32,476.32
	(20,347.23)	(23,966.60)
# of Weeks claimed for UI	15.81	27.80
	(14.76)	(29.85)
# of Weeks of Unemployment Duration	25.92	32.14
	(45.05)	(41.59)
Age	40.12	40.78
	(12.62)	(12.89)
White	88.49%	88.85%
	(0.32)	(0.31)
Male	62.61%	64.77%
	(0.48)	(0.48)
Years of Schooling	12.72	12.74
	(1.98)	(1.95)
# of Sampled unemployed	71,879	
# of Claims	95365	

Note: The maximum weekly benefit amount for eligible workers was \$401 for claims filed in 2006, which then increased to \$415. The lowest base-period earnings which qualified for the maximum benefits were \$30,662.18 and \$31,732.68, respectively. The weekly benefit rate payable to a compensated unemployed person is an amount equal to 1.3078% (the replacement rate) of his earnings in the base period, provided this is not more than the maximum amount.

to higher benefits, and hence enhance the value of employment with higher payments. Combining these two effects, the change in $p'(s_{b,d})$ is uncertain from equation (2), and hence the net effect on the search effort is ambiguous.

After hitting the threshold for the maximum benefit, i.e. $b = B_{max}$, equation (2) becomes:

$$p'(s_{B_{max},d}) = \frac{1}{\delta(V_E(w_{-1}, B_{max}) - V_U(B_{max}, d-1))}$$

The value of unemployment is then constant for all higher wages. The incremental benefit stops for the value of employment once the wage hits the threshold for the maximum UI benefit. Yet the value of employment continues rising due to increasing preceding job wages. Hence, $p'(s_{B_{max},d})$ will decline and search effort will increase after reaching the maximum benefit level. All in all, the chance of leaving UI for jobs will increase.

2.6 Empirical Specifications

The empirical challenge of identifying the effect of changes in UI benefits lies in the difficulty of finding a credibly exogenous and time invariant source of variations in UI benefits. Most sources of variations come from changes in legislations over time (Card and Levine (2000), Meyer (1990)). However, these changes are endogenous to labor market conditions. In this section, I use the kinked schedule in the relationship between previous earnings and benefit level to estimate the effect of UI benefits using administrative data from Kentucky. In contrast to traditional studies, mechanism of Regression Kink Design (RKD) disregards the effects from the labor markets. RKD is an empirical strategy which has been applied in the economic literature (Nielsen et al. (2010), Card et al. (2015), Card et al. (2012) and Landais (2013)), and offers valid non-parametric inference of the average treatment effect in the absence of instruments. I use a model in which the treatment is continuous and is a known deterministic function of the assignment variable, as in Landais (2013) and Card et al. (2015).

This type of setting can be characterized as a sharp design in the sense that everyone in the sample is treated and obeys the same treatment rule. The model is as follows:

$$Y = y(B, W, X, \varepsilon), \tag{2.3}$$

where Y is the outcome variable - unemployment duration. B is the continuous treatment variable - UI weekly benefit level, and determined solely by the assignment variable, W, equal to base period earnings. X are vectors that are determined prior to W as well as B. For instance, X include variables, such as ages, years of schooling and gender, which characterize individuals with C.D.F $F_X(x)$. ε is unobservable, non-additive error term[9]. If B is binary, then this model is equivalent to a framework where the "treatment effect" of B for a particular individual is given by $Y_1 - Y_0 = y(1, W, X, \varepsilon) - y(0, W, X, \varepsilon)$. Florens et al. (2008) defines the Average Treatment Effete (ATE) with respect to the continuous treatment of interest, which is an extension of the ATE in the binary treatment context, as follows:

$$\alpha = \frac{\partial \int y(B, W, X, \varepsilon) dF(w)}{\partial B}, \tag{2.4}$$

where $F(W)$ is the c.d.f. of W. The integration gives the average value of Y conditional on a particular pair of value for (b, x, ε). Conventional studies have proposed to use an instrumental variable ("IV") to identify causal parameters. An appropriate instrument variable is assumed to be correlated with B, and yet independent of the non-additive errors ε. However, when the regressor of interest is a policy variable which is determined by a endogenous variable, no IV can plausibly satisfy the "strong and unpalatable" assumptions. In my study, the unemployment benefits B are calculated as a fixed fraction of wage W up to some maximum benefit, and hence conditional on W there is no variation in the benefit level. Since wage is correlated with benefit, any variable that is independent of wage will be independent of the regressor of interest benefit, it is very unlikely to find instruments for benefit. Inspired by the regression discontinuity design, there are studies which try to identify a causal effect of B on Y using the kinked benefit rules. The idea is that if benefit B exerts a casual effect on Y, and there is a kink in the deterministic relation between B and W at w_{kink}, then there should be an induced kink in the relationship between Y and W at w_{kink}

As shown in Card et al. (2012), when the assignment rule has a kink at $W = W_{kink}$, the following assumptions are required to be satisfied:

Assumptions which make the RKD to be "as good as randomized" experiment:

(1). $y_1(b(w), w, x)$ exists for all $(b, w, x) \in R^3$, is integrable with respect to $F_X(x)$ for all $b \in R$, and is continuous in b for all x.

[9]This model allows for completely unrestricted non-additive heterogeneity in the response of Y to B.

(2). $f_{B|W,X}(b|w,x) = f_B(b)$ for all $(b,w,x) \in R^3$.

Intuitively, conditional on individuals' type, their probability of receiving certain amount of benefit is equal to the probability unconditional on any characteristics. In another word, each person faces the same possibility of receiving any level of benefit. Given this setting, the randomized experiment becomes that regardless personal characteristics, each individual will be randomly assigned weekly benefit at different amount, and then the outcome variable Y, unemployment duration, is observed.

Under the assumptions, Card et al. (2012) shows propositions which indicate that conditional on receiving certain level on UI benefit, the distribution of X is the same as the distribution for individuals regardless whether they are UI claimants or how much they receive from UI. Therefore, the assumptions is testable by examining the distribution of the covariates. The similarity of the distributions of the covariates with and without the treatment would support the validity of the RKD. This is an analogue of the conventional way to test whether an experiment is randomized.

Assumptions for the identification of RKD:

(1). $y_1(b(w),w,x)$ is continuous in b and $y_2(b(w),w,x)$ [10] is continuous in w for all b, w and x.

(2). $b(w)$ is continuous and continuously differentiable on $(-\infty,\infty)$ expect at point w_{kink} (i.e. $lim_{w\to w_{kink}^{+}} b^{'}(w) \neq lim_{w\to w_{kink}^{-}} b^{'}(w)$). Besides, $f_{W|X}(0|x)$ is strictly positive for $x \in A$, where $\int_A dF(x) > 0$.

(3). $F_{W|X}(w|x)$ is twice continuously differentiable in W at $W = W_{kink}$ for every x.

Card et al. (2012) derives propositions based on the required conditions for the identification of RKD. The proposition states that conditional on the assignment variable W, the rate of change in the distribution of X is continuous at the kink point w_{kink}. Given this smoothness condition, the change in the slope of $E[Y|W=w]$ at $w = w_{kink}$ divided by the change in the slope of b_W at the kink point identifies the $TT_{b_{w_{kink}}|w_{kink}}$. This is the same parameter identified by the randomized experiment, except that the $TT_{b_{w_{kink}}|w_{kink}}$ is evaluated at $B = b_{w_{kink}}$ and $W = w_{kink}$. At last, holding the three assumptions, the pre-determined characteristic X has continuously differentiable distribution conditional on the assignment variable W over the kink.

The average treatment effect of benefit level on the unemployment duration ($ATE_{b|w}$) can

[10] Define $y_2(b,w,x) \equiv \frac{\partial y(b,w,x)}{\partial w}$

be estimated by RKD as:

$$\alpha = \frac{\lim_{w \to w_{kink}^+} \frac{\partial E[Y|W=w]}{\partial w} - \lim_{w \to w_{kink}^-} \frac{\partial E[Y|W=w]}{\partial w}}{\lim_{w \to w_{kink}^+} \frac{\partial B(w)}{\partial w} - \lim_{w \to w_{kink}^-} \frac{\partial B(w)}{\partial w}} \tag{2.5}$$

where $B(w)$ is the benefit schedule which is formulaically determined by w, until w reaches w_{kink}. The key assumption used here is the $\frac{\partial E(B(W),W,X|W=w_{kink})}{\partial W}|_{b=B(w_{kink})}$ is continuous at w_{kink}. Intuitively, this condition ensures that a marginal change in W around the kink point only affects the unemployment duration Y through B, since the effects on W or other unobserved heterogeneity are symmetrically distributed around the kink point, and are eventually canceled out.

The denominator of the estimand, shown in equation (2.5), is determined by the UI schedule, which is the slope change of the benefit schedule at the kink. In my study, the slope of the benefit function changes from 0.013078 to 0 at the kink point; the denominator of the RK estimand is -0.013078. The estimation of the numerator is the change in the slope of the conditional expectation function of the outcome given the assignment variable at the kink. Using local polynomial estimation (Fan and Gijbels (1992)), Card et al. (2012) establishes consistency and asymptotic normality of local linear and local quadratic estimators for sharp kink designs under a set of regularity conditions. In my study I estimate the slope change by fitting parametric polynomial models:

$$lnY = \alpha + [\sum_{\rho=1}^{\rho=\bar{p}} \beta_\rho (W - W_{kink})^\rho + \theta_\rho (W - W_{kink})^\rho \amalg (W \geq W_{kink})] + \varepsilon, \text{ where } |W - W_{kink}| \leq h \tag{2.6}$$

where Y is the outcome variable, W_{kink} is the kink point in the benefit schedule, $\bar{p}$ is the maximum polynomial order, and h is the bandwidth size that determines the window $[W_{kink} - h, W_{kink} + h]$ within which the sample is selected. The change in the slope of the conditional expectation function is given by θ_1.

Previous literature both the maximum number of UI weeks and the maximum weekly benefit amount are determined by base-period earnings. The change in base-period earnings around the kink point can potentially change the benefit amount and the eligible benefit period simultaneously. In other words, the RK design cannot isolate the marginal effect of the benefit on the total number of weeks of UI claimed around the kink point. However, in my study such concern is alleviated by the policy adopted in Kentucky, where the weekly benefit amount is only affected by a change in base-period earnings during the sample period, while the number of eligible benefit weeks is determined by the time when the claimants filed their UI claims. Therefore, a straightforward RKD analysis will define a behavioral effect on the outcome in my case.

2.7 Graphical Evidence

Except for summarizing data, graphs can usually be used to test the validity of RK design assumptions. I first test the density of the assignment variable W which must be continuously differentiable at the kink point by plotting the probability density function of the base-period earnings, as shown in the Figure 2.2. To present these plots, I first divide the base period earnings into suitable bins, which is chosen by using the formal test of excess smoothing recommended by Lee and Lemieux (2010). According to the two kinks in the UI benefit schedules, I divide my sample into two groups. The first one includes all claims filed in 2006 that has the maximum benefit amount \$401, and the second group includes all the rest claims filed after 2007 with maximum weekly benefit \$415. Bin sizes of \$2759.6 and \$2538.61 are chosen for the two subgroups respectively. [11]

Each dot represents the number of observations in every bin correspondingly. Visually, there are no signs of discontinuity in the relationship between the number of observations and the assignment variable around the kink point. Even though the plot of 2006-2007 contains a bump on the right of the kink, the points go smoothly through the kink point. Statistically, the results from McCrary test, which is a standard test in the Regression Discontinuity Design literature, also confirm this conclusion. The estimate for the log changes in height of observations around the kink point and their bootstrapped standard errors are displayed on the graph. For the plot of 2006-2007, the Z-test statistics is -1.42 ($= -0.1/0.07$), which supports the continuous assumption on a 90% confidence interval. For the other plot of 2007 and after, the Z-test statistics is 0.67 (0.02/0.03), which even more strongly support the assumption.

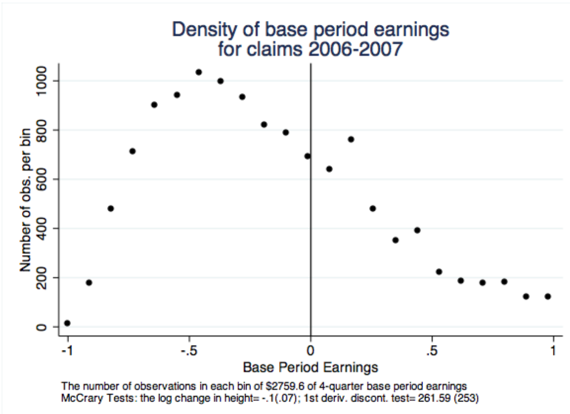
As done in Card et al. (2012), the McCrary test can be extended into a test for violation of the continuity of the derivative, by regressing the number of observations N_i in each bin on polynomials of the base-period earnings in each bin and the interaction term:

$$N_i = \alpha + \sum_{\rho=1}^{\rho=5} \beta_i (W - W_{kink})_i^{\rho} + \theta (W - W_{kink})_i \,\mathrm{II}\,(W \geq W_{kink}) + \varepsilon \tag{2.7}$$

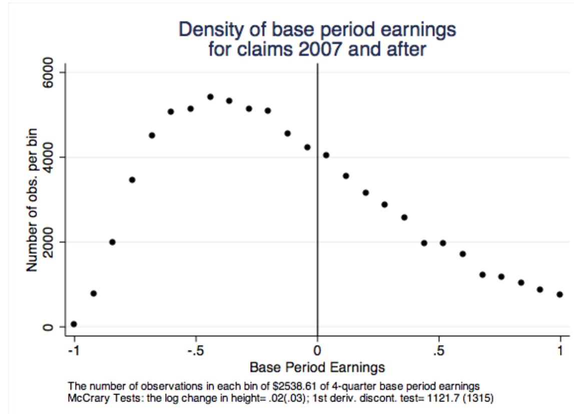
[11] Both of these bin sizes are the largest that pass the tests suggested by Lee and Lemieux (2010)

Figure 2.2: Density of Base Period Earnings

(a)



(b)



The coefficient of the interaction term θ tests the change in slope of the p.d.f. As reported in Figure 2.2, the estimates θ are 261.59 with standard error 253, and 1121.7 with standard error 1315, for 2006-2007 and after 2007 respectively, which suggest there is insignificant discontinuity in the derivative of the conditional density at the kink.

Second, the conditional expectation of any covariates should be continuously differentiable at the kink. This condition can be visually examined by plotting the mean values of covariates for observations in each bin against the assignment variable. In Figures 2.3 and 2.4, I show plots of conditional means of ages, years of education, the fraction of males, and the fraction of whites for different bins of base-period earnings in two subgroups that filed UI claims before and after 2007. All these covariates are predetermined before base-period earnings. The pattern of the years of education in Figure 2.3(b) is slightly bumpy to the left of the kink, but the magnitude of the jump is negligible. The mean values of age in claims after 2007 (Figure 2.4(a)) suggest some evidence of a kink to the right of the kink, but no strong visual evidence of discontinuities in the first derivative of the mean values at the kink point. Conducting the McCrary test, as in Figure 2.2, and the result confirms this conclusion. Inspections of all these plots suggest that the conditional means of the covariates go though the kink points smoothly, which supports the identification assumption of RK design.

Typically, the RKD analysis results can be summarized by the graphs as well. The potential kinks between the outcome variable of interest and the assignment variable – base-period earnings

Figure 2.3: Mean values of covariates in 2006

(a) (b)

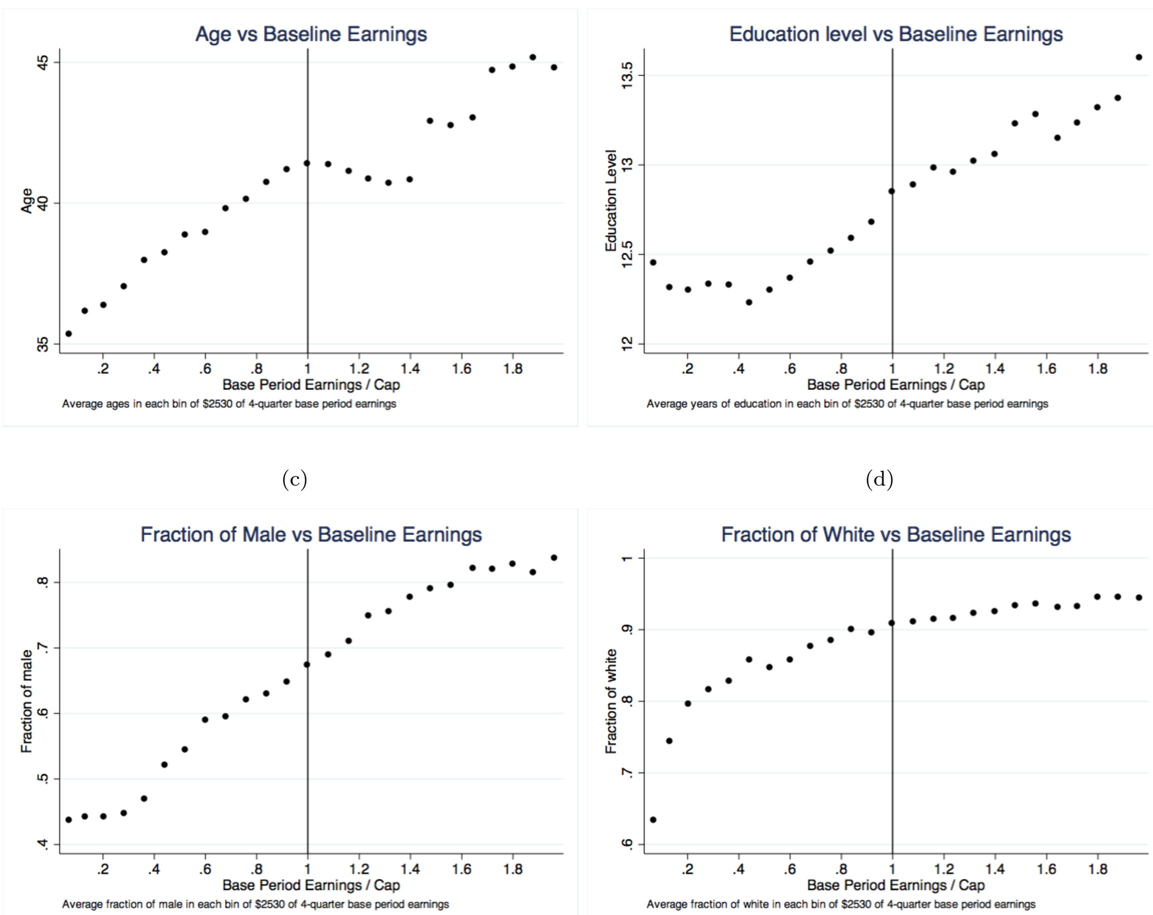
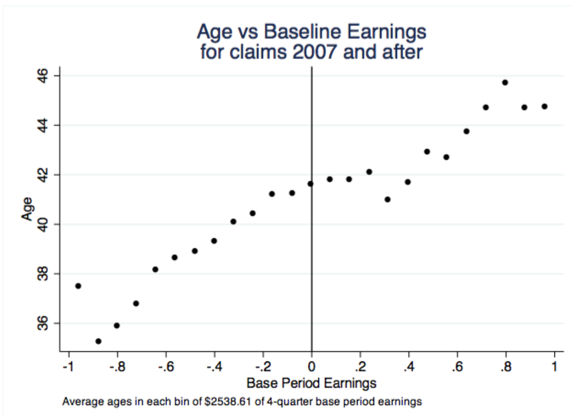
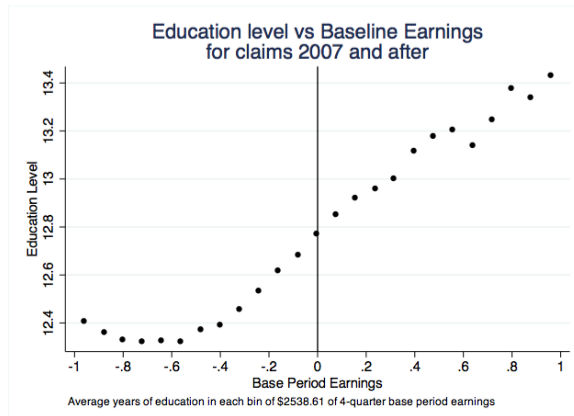


Figure 2.4: Mean values of covariates in 2007 and after

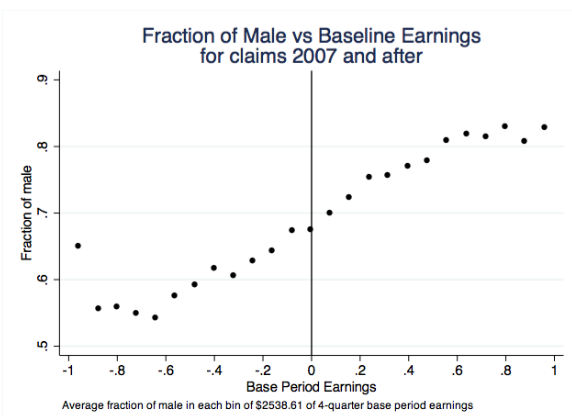
(a)



(b)



(c)



(d)

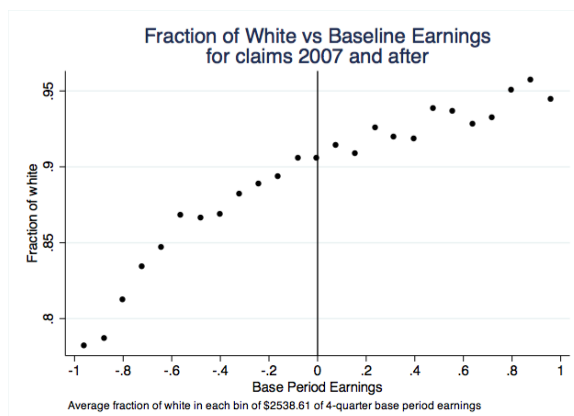
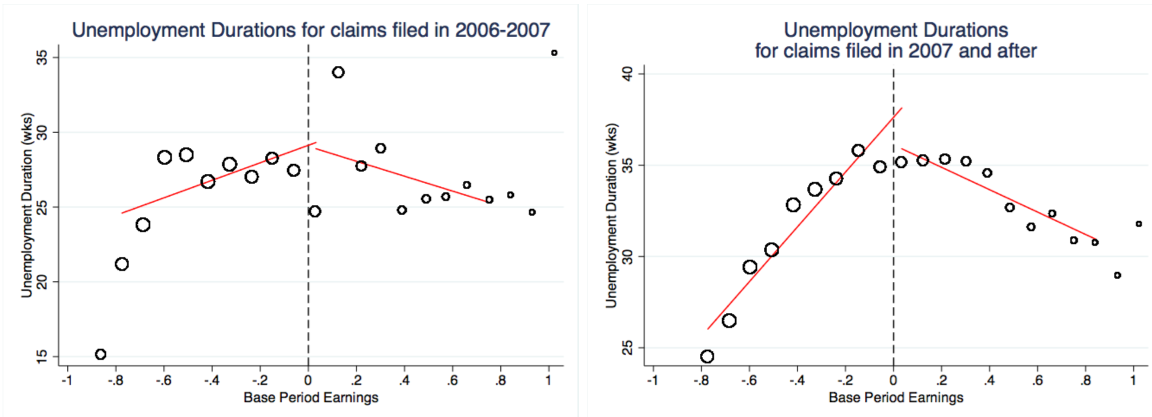


Figure 2.5: Mean values of outcome variable

(a) (b)



Notes: Each circle represents the weight-scaled mean in each bin of unemployment durations. The horizontal line represents the relative changes in prior earnings comparing to earnings on the threshold.

– are expected to be visualized around the kink points indicated in the formula. For two different subsamples, I plot the mean values in each bin of the outcome of interest, i.e. unemployment duration, against the assignment variable (presented in Figure 2.5). I limit my observation of base-period earnings with a range from 0 to \$61324.36 for the first subgroup and \$63465.36 for the second subgroup. These make the kink points of the base period earnings of \$30662.18 and \$31732.68 to be in the middle of each plot. The sharp changes in the slopes of the relationship between the outcome variables and the assignment variables at the kink points are observed in every plot. In each one, the outcome variables increase as the the assignment variable approaching to the kink point from the left, and then decrease as the assignment variable moving further to the right of the threshold. Figure 2.2(a) shows a more disturbing pattern for dots on the right of the kink, and the fitted line is less steep than the corresponding ones in other plots. Yet, there is still a clearly discernible change in the slope of the relationship between the two variables at the kink-point. All in all, these figures provide supportive evidence for the identification of an effect of benefit level on the unemployment duration in the RK design.

2.8 Estimation Results

Table 2.2 reports the effects on unemployment duration of the specification (2.6) with covariates. The point estimates of θ_1 represent the marginal impact of an extra dollar of prior-job weekly earnings on the average log unemployment durations. The elasticity of unemployment duration with respect to the weekly benefit level due to the kinked policy is calculated as follows:

$$\varepsilon_{kink} = \frac{\frac{\partial lnY}{\partial(W-W_{kink})}|_{kink^+} - \frac{\partial lnY}{\partial(W-W_{kink})}|_{kink^-}}{-\frac{\partial lnB}{\partial(W-W_{kink})}|_{kink^-}}, \tag{2.8}$$

where the numerator is the point estimate θ_1 from equation (2.6), and the denominator is the inverse of the W_{kink}.

I report the estimates from regressions up to a second order polynomial. The standard error are presented under each of the estimates. For each specification, the bandwidth is chosen based on the "rule-of-thumb" optimal bandwidth from Imbens and Kalyanaraman (2012).[12]

Comparing the estimates from groups which filed claims in 2006 and after 2007, unemployment duration elasticity with respect to benefits decreases significantly in the local linear specification. In 2006, increasing weekly benefits levels by 1% would increase unemployment duration by 0.52%, while increasing unemployment duration by 0.15 % for individuals who filed claims after 2007. The distinction between these two groups is the change in the maximum weekly benefit, from \$401 to \$415, and the corresponding thresholds of the minimum yearly earnings that could be eligible for the maximum weekly benefit, from \$30662.18 to \$31732.68. For the other weekly benefit change, which happened on December 21, 2008, every claim was given an extra \$25 each week, and yet the kink point in the benefit schedule is unchanged. To further inspect the benefit effect, I divide the sample which filed claims after 2007 into two subgroups, from 2007 to 2009 and after 2009. As shown in Columns (3) and (4) of Table 2.2, the estimates of unemployment elasticity from the linear specification decrease from 0.22 to 0.18, as the weekly benefit increased by \$25 for every claimant.

[12] The optimal bandwidth proposed in the paper is:

$$h_{opt} = C_K \Big(\frac{2\hat{\sigma}^2(c)/\hat{f(c)}}{(\hat{m}_+^{(2)}(c) - \hat{m}_-^{(2)}(c))^2} \Big)^{1/5} * N^{-1/5}$$

The density $f(c)$ and variance $\sigma^2(c)$ are estimated by assuming that the density and conditional variance functions are continuous at the threshold c. The second derivative $\hat{m}_+^{(2)}(c)$ is estimated by fitting a quadratic function to the observations with $X_i \in [c, c+h]$, and similarly $\hat{m}_-^{(2)}(c)$ is estimated by fitting a quadratic function to the observations with $X_i \in [c-h, c]$. C_K is a constant that depends on the kernel used. For the triangular kernel, with $K(u) = (1 - |u|) * I(|u| \leq 1)$, the constant is 3.4375.

Table 2.2: RKD estimate of UI benefit on Unemployment Durations

	2006	2007 and After	2007 - 2008	2009 and After
	Local Linear			
$\hat{\theta}_1$	-8.878×10^{-4}	-2.389×10^{-4}	-3.635×10^{-4}	-2.928×10^{-4}
	(1.854×10^{-4})	(2.217×10^{-4})	(1.337×10^{-4})	(2.162×10^{-4})
P–value	0.8044	0.3753	0.1174	0.9373
AIC	29891	103524	70151	36425
Elasticity	0.5235	0.1458	0.2218	0.1787
	(0.1093)	(0.1353)	(0.0816)	(0.1319)
Bandwidth	28784	12142	21982	18233
# Obs	9534	31484	23036	11853
	Local Quadratic			
$\hat{\theta}_1$	-2.27×10^{-4}	1.64×10^{-5}	6.62×10^{-4}	-7.639×10^{-4}
	(5.638×10^{-4})	(2.962×10^{-4})	(3.905×10^{-4})	(3.503×10^{-4})
P–value	0.0156	0.9699	0.5100	0.8230
AIC	30613	187054	77162	52912
Elasticity	0.1338	-0.0100	-0.4040	0.4662
	(0.3324)	(0.1808)	(0.2383)	(0.2138)
Bandwidth	35618	27128	26994	49857
# Obs	9762	56826	25309	17097

Note: The point estimates of $\hat{\theta}_1$ represent the marginal impact of an extra dollar of prior-job weekly earnings on the average log unemployment durations over the kink. The bandwidth is chosen based on the "rule-of-thumb" optimal bandwidth from Imbens and Kalyanaraman (2012). Smaller (lower than 0.05) p-values from Goodness-of-Fit tests suggest that the estimates from the corresponding specifications are robust against the choice of bin size at a 95% confidence interval. Among different polynomial models for each group, the smaller AIC suggests the order preferred.

The maximum weekly benefit (kink point) shifts from$401 to $415 from the 2006 group to the 2007 and after group, while the benefit schedule shifts up by $25 each week for claims filed in and after 2009 compared with claims filed in 2007 and 2008.

To closely inspect the relationship between elasticity estimates and bandwidth, in Figure 2.6 I plot the local linear estimates (the best specifications according to the AIC) for different samples associated with a range of bandwidths from \$100 to \$1000 weekly prior-job earnings. As expected, the RKD does poorly with small samples, and the estimates become more precise as the bandwidth increases, while the standard errors get smaller. At the optimal bandwidth, the elasticity estimate for claims in 2006 is much bigger than the elasticity estimate for claims in 2007 and after. However, as the bandwidth increases, the elasticity estimates from the two groups converge. The elasticity in the group after 2007 converges to 0.45 after a bandwidth of \$400. In contrast, the elasticity estimates for claims in 2007-2009 are always larger than those in the group for claims after 2009. The pattern is more striking after the chosen optimal bandwidth. In general, the optimal bandwidth produces the most meaningful estimates.

Table 2.2 presents p-values from a Goodness-of-Fit test and Aikake Information Criterion (AIC) for each specification. In each group, none of the p-values is less than 0.05 in any of the linear specifications[13], which suggests that the linear specification is not sensitive to the inclusion of the bin dummies, and hence the estimates are robust against choosing different bin sizes. The reported AIC, which is used to select the preferred order of the polynomial, suggest that the linear specification is dominated in every group.[14]

It is notable that in Table 2.2, the inclusion of the quadratic term causes the marginal effect of the weekly benefit level on the unemployment duration $\hat{\theta}_1$ to change signs and become statistically insignificant for subgroup which filed claims after 2007 and for subgroup which filed claims in 2007-2009. There is a lot of curvature before the kink point, which is captured by fitting the quadratic term. The linear trend corrected the over-fitted curvature by the quadratic term after the kink. Since the trend of unemployment durations become flatter after the kink point relative to the trend before the kink point, the estimates of the linear term $\hat{\theta}_1$ become positive.

To more accurately obtain labor supply effects with respect to unemployment durations, I investigate the effect of different UI claimant characteristics. The estimates are generally consistent across demographic groups, except for the age group. Figure 2.7 shows the relationship between unemployment duration and base-period earnings among people under 26 years old. For claimants

[13]The p-values are from a Goodness-of-Fit test that compares the polynomial model to the same polynomial model with a series of bin dummies.

[14]My conclusion can be limited by the bandwidth chosen. Landais (2013) shows that the larger the bandwidth, the less likely the linear specification will dominate higher order polynomials.

Figure 2.6: Elasticity of Unemployment Duration w.r.t. benefit

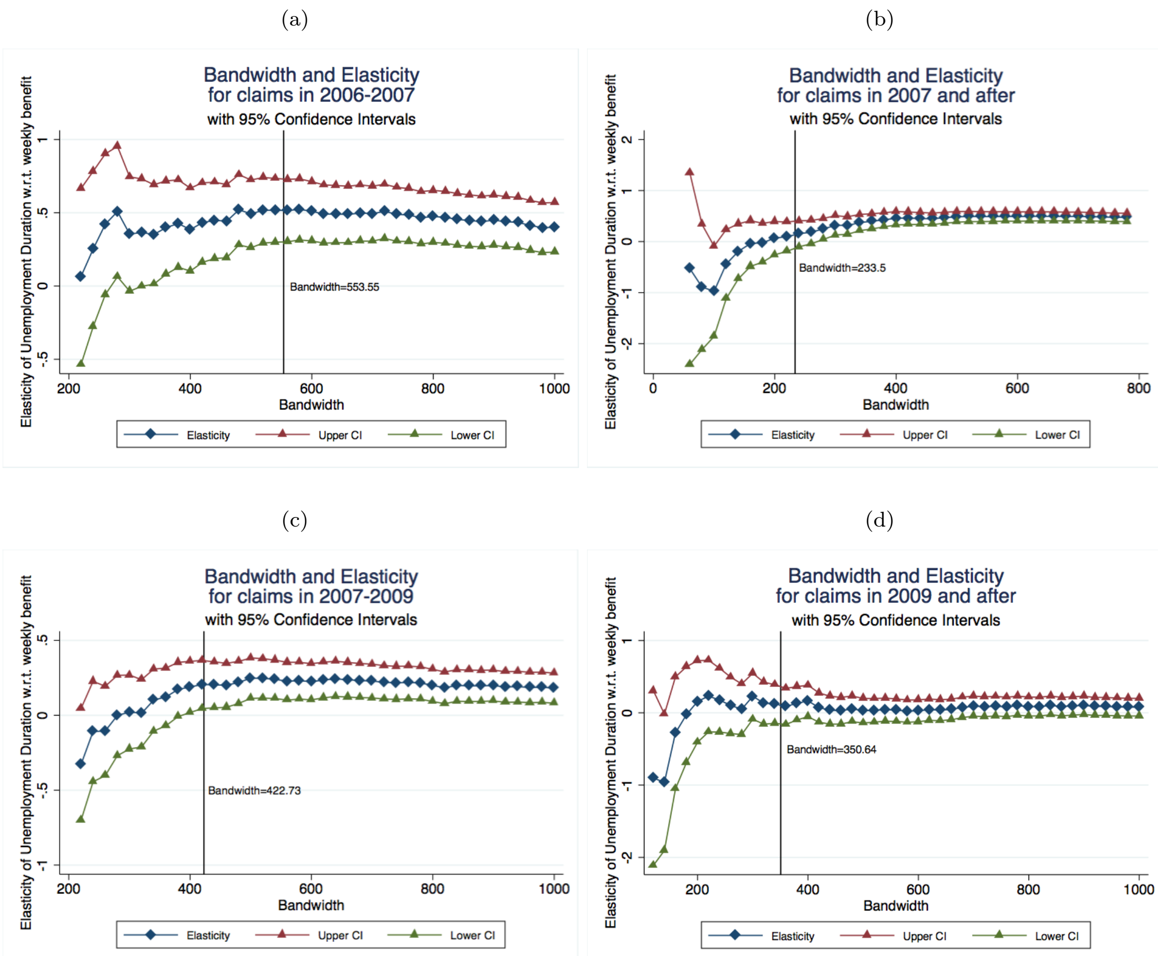
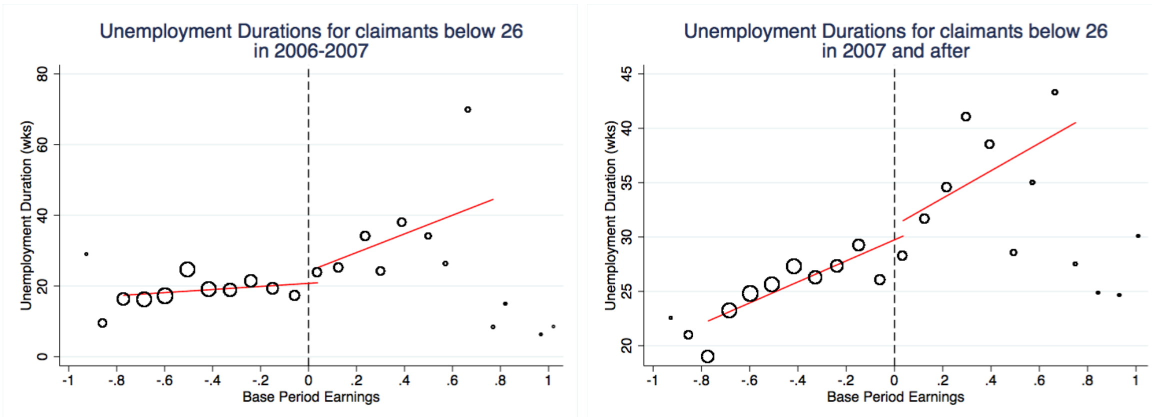


Figure 2.7: Unemployment Durations for Different Age Groups

(a) (b)



above kink points with maximum benefits, unemployment durations increase as prior-earnings rise, and the increasing rates are even higher than the observations below the kink points. This pattern is the opposite of that observed across the entire sample (as shown in Figure 2.5): As the replacement rate drops over the kink, the opportunity cost of staying unemployed increases, and hence the unemployment duration does not drop sharply at the exact point of the cutoff, but rapidly decreases over a narrow range of values just below the cutoff, as predicted in the proposition. Table 2.3 shows the RKD estimates for the two age groups. The estimates are consistent across different specifications. The AIC suggests that the linear estimates dominate in each period.

Unlike older people, typical young people without long work histories would have had less access to loans in the past, and hence have a lower stock of debt. Therefore young people's liquidity constraints are lower. As long as the UI benefit covers young people's basic living expenses, they are more patient about searching for and accepting new jobs. In addition, young people have lower human capital depreciation rates than old people, and will generally tend to wait for a "better" job which could help them improve their lifetime career prospects and build up human capital, the marginal return of which is also higher for young people. This is even more true for young unemployed people who used to earn high wages (higher than the kinked scheduled wages) and tend to have higher expectations about their future jobs. High-earning youth unemployed people would also have a smaller market due to the high skill levels they possess, and they may also overestimate their skills and mismatch themselves with the market. Combining all these effects, young people's unemployment durations can increase along with the prior-earnings. It is also noticeable that the marginal increase could be higher for higher-earning unemployed young people than lower-earning ones. Table 2.3 shows the estimates of elasticities for the young and old age groups, and the estimates for the young group are always negative across different periods and different specifications. These markedly different estimates from different age groups suggest that the effect of unemployment insurance benefit could vary over the lifecycle.

Generally, my estimates range from 0.2 to 0.5, which are at the lower end of estimates in the literature on US data: Chetty (2008) indicates that the elasticity of UI benefit on unemployment duration is between 0.7 and 1; Meyer (1990) finds an elasticity of 0.56. One explanation is that I have employed a new method, RKD, which is different from the analysis used in conventional studies, and intend to solve the traditional policy endogeneity issue. The generosity of UI usually increases when unemployment is high, and hence policy endogeneity will make unemployment duration elasticity artificially larger during times of high unemployment. In the next section, I conduct the analysis using the traditional method, and the results are larger than my RKD estimates. In addition, my estimates are closer to Landais (2013), which also uses RKD to estimate unemployment duration elasticity with respect to benefit level. As suggested by Mitman and Rabinovich (2015) and Schmieder et al. (2012), the disincentive effect of unemployment benefits could be different over the business cycle. Kroft and Notowidigdo (2011) uses different simulation strategies to solve the policy endogeneity issue and finds that the duration elasticity is smaller when the local unemployment rate is relatively high. This is supported by my empirical results and will be discussed in the next section.

2.9 Robustness Check

2.9.1 Comparison to the results from Hazard model

The biggest concern with using the traditional hazard model in the UI study is endogeneity, and the common way to mitigate it is to add a set of controls. In Table 2.4, I present the estimates of the effect of UI weekly benefit levels on the hazard rate of exiting UI for jobs from 5 different specifications with various controls, using the sample after 2007. Generally, the estimates from traditional hazard models are higher than the estimates from RKD as discussed before.

In all of them, I include controls for gender, ethnicity, age, industries, years of schooling,

Table 2.3: Estimates for different age groups

	2006	2007 and After	2007 - 2008	2009 and After
Young($\leq$26)				
	Local Linear			
$\hat{\theta}_1$	1.437×10^{-4}	1.3455×10^{-3}	8.253×10^{-4}	6.351×10^{-4}
	(6.006×10^{-4})	(4.167×10^{-4})	(4.615×10^{-4})	(4.557×10^{-4})
Elasticity	-0.0847	-0.8211	-0.5036	-0.3875
	(0.3541)	(0.2543)	(0.2816)	(0.2781)
# Obs	1683	6256	3449	2640
AIC	5301	20628	10787	8230
	Local Quadratic			
$\hat{\theta}_1$	9.297×10^{-4}	3.0476×10^{-3}	2.4894×10^{-3}	9.291×10^{-4}
	(1.8416×10^{-3})	(7.414×10^{-4})	(9.26×10^{-4})	(1.2014×10^{-3})
Elasticity	-0.5482	-1.8598	-1.5191	-0.5670
	(1.0859)	(0.4524)	(0.5651)	(0.7331)
# Obs	1688	10345	4763	3061
AIC	5320	34216	14965	9577
Old(>26)				
	Local Linear			
$\hat{\theta}_1$	-4.71×10^{-4}	-8.809×10^{-4}	-3.569×10^{-4}	-1.643×10^{-4}
	(1.385×10^{-4})	(1.204×10^{-4})	(9.83×10^{-5})	(1.748×10^{-4})
Elasticity	0.2777	0.5375	0.2178	0.1003
	(0.0817)	(0.0735)	(0.0600)	(0.1067)
# Obs	8419	40693	21840	11545
AIC	24256	133533	63007	35363
	Local Quadratic			
$\hat{\theta}_1$	-2.19×10^{-5}	-5.441×10^{-4}	-1.49×10^{-5}	-7.881×10^{-4}
	(6.433×10^{-4})	(2.856×10^{-4})	(3.893×10^{-4})	(4.585×10^{-4})
Elasticity	0.0129	0.3320	0.0091	0.4809
	(0.3793)	(0.1743)	(0.2376)	(0.2798)
# Obs	7919	48097	21052	13218
AIC	26692	157704	66575	40647

Table 2.4: Use Cox-proportional Hazard Models to estimate the effect of UI benefit

Variables	(1)	(2)	(3)	(4)	(5)
I(Remaining benefit week>0)	-1.192***	-1.177***	-1.178***	-1.182***	2.519***
	(0.0945)	(0.0937)	(0.0937)	(0.0938)	(0.3760)
Remaining Benefit weeks	-0.00665***	-0.00649***	-0.00647***	-0.00652***	-0.00617*
	(0.0021)	(0.0021)	(0.0021)	(0.0021)	(0.0035)
ln_benefit	-0.318***	-0.676***	-0.657***		
	(0.0457)	(0.0744)	(0.0744)		
ln_wages		0.355***	0.276***	0.355***	0.285***
		(0.0530)	(0.0622)	(0.0532)	(0.0470)
ln_benefit*(above the kink)			0.0203**		
			(0.0084)		
lnb*(ur<=0.09)				-0.612***	
				(0.0807)	
lnb*(0.09<ur<=0.1)				-0.532***	
				(0.0820)	
lnb*(0.1<ur<=0.11)				-0.576***	
				(0.0927)	
lnb*(0<wksleft<=6)					-0.607***
					(0.0667)
lnb*(6<wksleft<=29)					-0.676***
					(0.0638)
lnb*(29<wksleft<=33)					-0.685***
					(0.0646)
lnb*(33<wksleft<=53)					-0.679***
					(0.0634)
lnb*(53<wksleft<=73)					-0.653***
					(0.0648)
lnb*(73<wksleft)					-0.661***
					(0.0693)
New Claim Rates	-12.01	-11.85	-11.77	-11.92	-11.22
	(11.64)	(11.63)	(11.63)	(11.63)	(11.63)
Constant	0.948	-0.655	-0.00505	-0.598	-0.4009
	(0.5830)	(0.5910)	(0.6590)	(0.6480)	(0.5740)
Observations	362,839	362,839	362,839	362,839	362,839
	LR test of Gamma	LR test of Gamma	LR test of Gamma	LR test of Gamma	LR test of Gamma
	chibar2=8.9961	Chibar2=36.21	Chibar2=37.24	Chibar2=37.308	chibar2=32.7191
	P-value=0.001357	P-value=8.9E-010	P-value=5.8E-010	P-value=5.10E-010	P-value=5.3E-009

Note: In these specifications, I control for fixed time effects for calendar month, three-order polynomial insured unemployment rates, three-order polynomial new UI claims rates, and baseline hazard. I treat the baseline hazard non-parametrically by creating 100 interval-specific dummy variables (one for each week at risk), as the longest observed UI spell in my data set is 99 weeks and the shortest is 0. ***(**) (*) indicates statistical significance at the 1% (5%) (10%) level.

Variables	1	2	3	4	5
Male	0.306***	0.263***	0.261***	0.265***	0.252***
	(0.0387)	(0.0377)	(0.0377)	(0.0378)	(0.0365)
White	0.0439	0.026	0.0259	0.0251	0.0256
	(0.0541)	(0.0529)	(0.0530)	(0.0531)	(0.0509)
Age					
22-31	-0.374***	-0.342***	-0.342***	-0.344***	-0.337***
	(0.0770)	(0.0750)	(0.0751)	(0.0753)	(0.0726)
32-41	-0.514***	-0.500***	-0.501***	-0.502***	-0.487***
	(0.0813)	(0.0793)	(0.0794)	(0.0796)	(0.0770)
42-51	-0.684***	-0.680***	-0.681***	-0.682***	-0.661***
	(0.0831)	(0.0812)	(0.0812)	(0.0814)	(0.0791)
52-62	-1.047***	-1.041***	-1.041***	-1.044***	-1.013***
	(0.0893)	(0.0872)	(0.0871)	(0.0874)	(0.0854)
Years of Education					
8	-0.0936	-0.0505	-0.0387	-0.0513	-0.0702
	(0.1930)	(0.1890)	(0.1890)	(0.1900)	(0.1820)
9	-0.142	-0.101	-0.0908	-0.101	-0.0984
	(0.1390)	(0.1360)	(0.1360)	(0.1360)	(0.1300)
11	-0.158*	-0.115	-0.108	-0.116	-0.12
	(0.0949)	(0.0929)	(0.0931)	(0.0933)	(0.0894)
12	0.107**	0.141***	0.143***	0.141***	0.129***
	(0.0486)	(0.0479)	(0.0480)	(0.0481)	(0.0462)
15	0.088	0.105	0.105	0.105	0.0999
	(0.0874)	(0.0854)	(0.0855)	(0.0857)	(0.0825)
16	0.260***	0.210***	0.209***	0.213***	0.208***
	(0.0717)	(0.0704)	(0.0705)	(0.0707)	(0.0683)
18	0.279**	0.167	0.168	0.17	0.17
	(0.1260)	(0.1250)	(0.1250)	(0.1250)	(0.1210)
20	0.0144	-0.137	-0.126	-0.133	-0.12
	(0.4270)	(0.4200)	(0.4200)	(0.4210)	(0.4070)

Variables	1	2	3	4	5
Industry					
Accom.-Food Services	0.912**	0.737*	0.736*	0.736*	-0.418
	(0.4290)	(0.4210)	(0.4210)	(0.4220)	(0.4130)
Agriculture	0.652	0.573	0.577	0.569	-0.396
	(0.4100)	(0.4030)	(0.4030)	(0.4040)	(0.6360)
Construction	0.407	0.315	0.327	0.311	0.137
	(0.4130)	(0.4050)	(0.4060)	(0.4060)	(0.1510)
Finance and Insurance	0.587	0.526	0.532	0.523	-0.119
	(0.4120)	(0.4050)	(0.4050)	(0.4060)	(0.1540)
Health Care	-0.0172	-0.0772	-0.0668	-0.0833	0.102
	(0.4240)	(0.4170)	(0.4170)	(0.4180)	(0.1600)
Management of Companies and Enterprises	0.092	0.0376	0.0538	0.0327	-0.0374
	(0.4260)	(0.4180)	(0.4180)	(0.4190)	(0.1670)
Manufacturing	0.357	0.272	0.283	0.269	-0.361*
	(0.4120)	(0.4050)	(0.4050)	(0.4060)	(0.1910)
Mining	0.589	0.379	0.401	0.374	-0.138
	(0.4510)	(0.4430)	(0.4440)	(0.4440)	(0.1610)
Professional, Scientific, and Technical	0.156	0.105	0.118	0.0969	-0.0471
	(0.4560)	(0.4480)	(0.4480)	(0.4490)	(0.1500)
Public Administration	0.586	0.528	0.545	0.526	-0.303
	(0.4100)	(0.4030)	(0.4030)	(0.4040)	(0.2460)
Real Estate -Rental and Leasing	0.0535	0.0144	0.0252	0.01	0.105
	(0.4260)	(0.4180)	(0.4190)	(0.4200)	(0.1550)
Retail Trade	0.659	0.604	0.612	0.603	-0.380**
	(0.4080)	(0.4010)	(0.4020)	(0.4020)	(0.1930)
Transportation and Warehousing	0.489	0.436	0.449	0.432	0.186
	(0.4110)	(0.4040)	(0.4040)	(0.4050)	(0.1520)
Utilities	0.581	0.559	0.576	0.557	0.0217
	(0.4910)	(0.4820)	(0.4830)	(0.4840)	(0.1580)
Wholesale Trade	0.507	0.42	0.431	0.416	0.132
	(0.4340)	(0.4260)	(0.4260)	(0.4270)	(0.3000)

new claims rates, and calendar month fixed effects. Coefficient estimates for lnb in the proportional hazard models can be interpreted as the elasticity of the hazard rates with respect to the weekly benefit. Presumably, the exit hazard rate is constant, and hence the unemployment duration is the inverse of it. $Y = \frac{1}{\text{Exit Hazard}}$. The elasticity of unemployment duration with respect to the weekly benefit can be written as $\varepsilon_Y = \frac{\partial lnY}{\partial lnb} = \frac{\partial ln(\frac{1}{\lambda})}{\partial lnb} = -\frac{\partial ln(\lambda)}{\partial lnb} = -\varepsilon_\lambda$, where the Y is the unemployment duration and λ is the exit hazard rate.

In the first specification, as shown in the first column of Table 2.4, the elasticity estimate of exit hazard is -0.318 (The corresponding elasticity of unemployment duration is 0.318), which is larger than the RKD estimated elasticity of unemployment duration 0.15, shown in Table 2.2 Column 2. The traditional endogeneity issue would be concerned in the first specification due to the fact that the benefit is determined by the prior earnings. In the second specification, I include the log-base-period earnings, and the elasticity estimate increase to -0.676, which is slightly bigger than the upper bound of the RKD estimates (-0.45) conditional on the bandwidth bigger than \$400. After adding in an interaction term of log benefit with over the kink, the coefficient of the lnb doesn't change much, shown in the Table 2.4 Column 3, but the coefficient of the interaction term is positive. The positive estimate implies that for claimants who used to earn wages higher than the cut off \$31732.68, a increment of the weekly benefit would increase their possibility of finding jobs, and lower the unemployment duration. This is consistent with the pattern in Figure 2.5(b).

It is worth mentioning that the Great Recession is documented from December 2007 to June 2009, which coincides with when the kink in benefit schedules shifted (2007) and when a lump sum amount of benefit was added into each claim (2009). Since UI generosity, in terms of both benefit level and duration, varied with the business cycle, the estimates from different periods might include effects from cyclical changes.[15] To detect the cyclical effect, in one of my hazard model specifications, I include interaction terms of log benefit with different unemployment rates, as show in column(4). My results suggest that the negative effect of the benefit level on the exit hazards decreases as unemployment rates increases. This is consistent with my RKD results from two subgroups of claims filed between 2007-2009 (with unemployment rates 6% - 7%) and claims filed after 2009 (with unemployment rates 7% - 11%). The RKD estimates decrease significantly from the earlier period to the later period, which indicates that as the recession got worse and unemployment grew, the elasticity of the unemployment duration with respect to the benefit level

[15] See Schmieder et al. (2012) and Kroft and Notowidigdo (2011).

decreased as well.

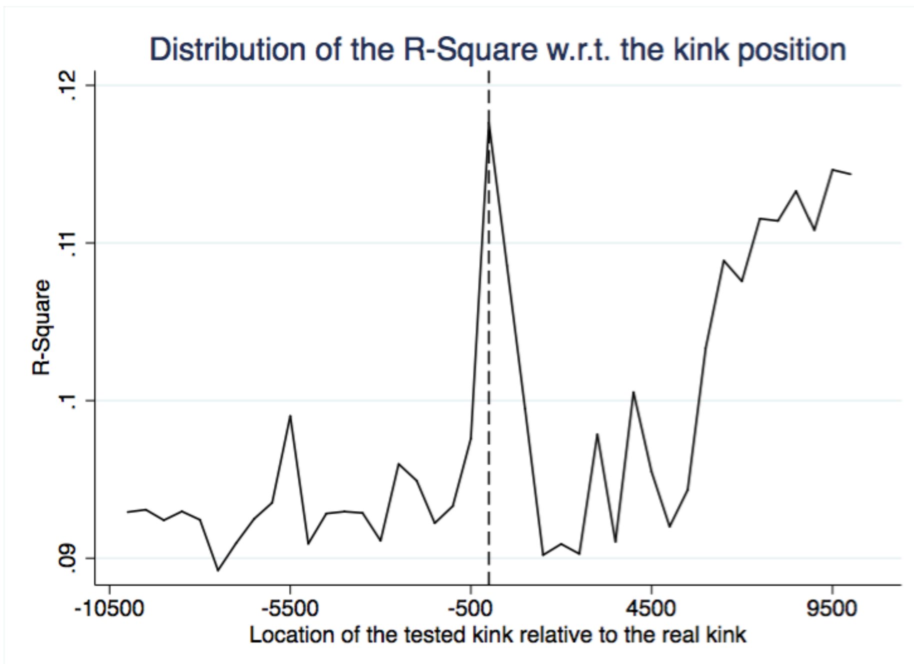
2.9.2 Check the location of the kink

Since RKD relies on the pattern of the data around the kink point to identify the treatment effect, the RKD estimate might include some outliers in the relationship between the assignment variable W and the outcomes Y. These outliers disturb the estimated results. Following Landais (2013), I conduct a robustness test to detect the location of the kink which would minimize the residual sum of squares or, equivalently, maximize R-squared. In principle, the "empirical kink" should be coincident with the real kink in the benefit schedule, and hence it is worth seeing whether the two kinks are the same in my study. The test is conducted by running the RKD specification, equation (4.6), for multiple choices of kink points, and then comparing the corresponding R-squared from changing the location of the kink point. Figure 2.8 shows the R-squared against the location of the kink point. The dashed line shows the location of the real kink, indicated by the benefit schedule. The R-squared increases sharply as the tested kink location approaches the real kink location, and then slumps. Even though after the tested kink \$41232.68 (\$ 9,500 at the right of the real kink \$31732.68) the R-squared increases again, the bandwidth (\$21573.46) includes more than 51,000 out of 59,940 observations in the sample. Since the majority of observations are covered in the regressions with large bandwidth, the effect from the outliers is mitigated and R-squared increases subsequently. The kink point that maximizes R-squared is located at the real kink point, and this provides strong evidence for the validity of the RKD.

2.10 Conclusion

In this paper, I use administrative data from Kentucky to examine two dimensions of the effect of UI benefits on unemployment duration during the Great Recession. Using the kinks in the schedule of UI benefits, I identify the effect of UI benefit by adopting RKD analysis. My estimates suggest the elasticity of unemployment spells with respect to benefit level lies in the range from 0.2 to 0.5, which is on the lower end of estimates from the existing literature on US data. Since the traditional methods do not effectively solve the policy endogeneity issue, and the unemployment duration elasticity is artificially larger during times of high unemployment. My result can be treated as evidence that RKD could theoretically solve the traditional endogeneity issue, and hence the

Figure 2.8: Distribution of R^2



Notes: The horizontal line represents the relative position of the tested kink against the real kink in the benefit schedule, which is indicated by the dashed line. The real kink is $31732.68.

policy effect derived from this method is presumably lower than conventional studies suggest.

My results also suggest that the cyclical effect of the disincentive effect of benefit level on unemployment duration decreases as unemployment rates increase, which coincides with conclusions in Mitman and Rabinovich (2015), Schmieder et al. (2012) and Kroft and Notowidigdo (2011).

It worth mentioning that in my study youth unemployment exhibits quite different unemployment duration patterns than for workers older than 26: As the replacement rate drops, unemployment durations increase, and the increasing rates are even higher for those with lower wages.

Appendices

Appendix A Literature Review

The most recent literature, which investigates the effects of the extended UI benefits on the labor market, uses the Current Population Survey (CPS) data. The CPS visits a given address (housing unit) for four months, does not interview for eight months, and revisits the address for four more months. This sample structure allows researchers to match households in consecutive months up to three times. As confirmed in both Farber and Valletta (2013) and Rothstein (2011), the unemployment durations implied by the frequency and duration structure of exits from unemployment are much lower than the reported durations of spells in progress in the CPS cross-section. To deal with the spurious exits from unemployment, both papers recode the intervening month as a continuation of the initial unemployment spell. For instance, for individuals who report a transition out of unemployment in month one followed by an entry back to unemployment in month three (i.e. UEU or UNU, where U unemployment, E employment, and N non-labor force), both papers recode the middle month as U unemployment. Since the structure of the CPS data doesn't allow researchers to match households after the fourth month, the recoding method cannot be applied to individuals who transit out of unemployment in month four. In another word, imposing this adjustment to observed transitions requires restricting the set of observations to those from the first two of each set of four consecutive CPS rotation groups. To have complete information, only at least two subsequent matched observations are sampled and each individual needs to be observed at least 3 of four months in both papers.

As opposed to these two papers, Fujita (2011) uses a different method to deal with the same issue. When a worker is unemployed for two consecutive months, the CPS procedure automatically assumes that the worker is continuously unemployed with no intra-month employment spell. When a worker reported employed or NILF (Not In the Labor Force) in the previous month's survey, the worker cannot logically be unemployed for more than 5 weeks if he reports that he is unemployed in this month. When this logical inconsistency is observed in the data (for example, a worker reports that his unemployment duration is 26 weeks even though he reported NILF in the previous month's survey), Fujita (2011) assume that the previous month's labor market status was misreported, and corrected data by assuming the worker with an inconsistent transition to be unemployed in the previous month.

Due to the fact of losing direct information in the CPS on UI eligibility, and most papers rely on the reported reason for unemployment to measure the eligibility. Unemployed individuals who report having lost a job as the reason for unemployment are, in principle, eligible to receive UI. In contrast, individuals who report voluntarily leaving a job or new-entry into the labor force are, in principle, not eligible to receive UI. Both Farber and Valletta (2013) and Rothstein (2011) classify unemployed Job Losers as the UI-eligible group, and Job Leavers and new labor force entrants as UI-ineligible group.

Each March, the regular monthly CPS is accompanied by an extensive set of supplemental questions regarding income in the previous calendar year, and UI is identified as a source of income. The rotating sample structure of the CPS enables matching of observations on unemployed individuals for selected months in year t with the information on their income receipt in year t recorded in the year $t + 1$ March supplement. Based on this match, Farber and Valletta (2013) provides fractions of unemployed individuals from monthly CPS who report receiving annual UI income in subsequent matched March CPS for both UI-eligible group and UI-ineligible group. The percentage of the UI-eligible who report receiving UI income reached about 50 percent, while the percentage of the UI-ineligible group is about 5-10 percent. This fact is also confirmed by Rothstein (2011). Only half of individuals sampled in both papers are actual UI claimants, and there is high chance that the estimates are largely biased.

Rothstein (2011) matches over 70% of monthly respondents to employment statuses in the following month, measures the one-month-later employment outcomes for roughly 4,000 unemployed workers, and constructs monthly reemployment rates and labor force exit rates. All these sampled workers, who are observed to exit unemployment in one month but return the following month, have at least two subsequent interviews. Given the assumption that all displaced workers are eligible for full benefits, Rothstein (2011) simulates remaining benefit durations for each of them.

According to weekly dates of the availability of EUC and EB benefits for different states, Rothstein (2011) computes the eligibility for benefits in each week between the time of being unemployed and the time of initial interviews, and the available benefit weeks decrease one week by each week, while assuming that individuals anticipate no further legislative changes of benefits.

Using the matched CPS data, Rothstein (2011) assumes that the monthly hazard of leaving unemployment follows a logistic function. Let λ_{ist} be the probability that the individual i in state

s exits unemployment by month $t+1$:

$$ln(\frac{\lambda_{ist}}{1-\lambda_{ist}}) = \theta D_{ist} + P_n(n_{ist};\gamma) + P_Z(Z_{st};\delta) + \alpha_s + \eta_t, \tag{9}$$

where D_{ist} the total number of weeks of benefits available to individual i, including the n_{ist} weeks already used as well as weeks she expects to be able to draw in the future, and Z_{st} is a measure of economic conditions. α_s and η_t are fixed effects for states and months, P_n and P_Z are flexible polynomials.

Since the UI got extended when the labor market became worse, and the total number of benefit weeks available to each person varies with labor demand conditions, Rothstein (2011) proposes several strategies to identify the different components of the variation in the benefit weeks, which are exogenous to unobserved determinants of the unemployment hazard.

The first strategy aims at absorbing labor demand condition through including a cubic polynomial function P_Z, in the state unemployment rates. The remaining variation in the total number of benefit weeks primarily comes from the repeated expiration and renewal of the EUC, and whether the EB are triggered in different states. Usually, the extension of benefits corresponds to the weak labor demand, and this negative correlation intends to overstate the casual effects of the UI benefits on unemployment exit. The implied effects of UI expansions on the exit hazard, which is computed as the difference between the average fitted exit probability and the average fitted probability implied by the model with benefit durations set to 26 weeks for the entire sample, range from -1.7 to -2.3 percentage points in 2010:Q4.

The second strategy uses a control group, which include unemployed workers not eligible for UI, i.e. job-leavers, to control the state labor market conditions (This method is also used in Valetta and Kuang, 2010; Farber and Valletta, 2011), without needing to control the effects of the employment rates across different states.

$$ln(\frac{\lambda_{ist}}{1-\lambda_{ist}}) = \omega D_{ist} + e_{ist}\theta D_{ist} + P_n(n_{ist}, e_{ist};\gamma) + e_{ist}P_Z(Z_{st};\delta) + \alpha_{st}, \tag{10}$$

where e_{its} is an indicator for whether the individual i is a job loser (UI-eligible), and α_{st} is a full set of state-month indicators.

However the proportion of job leavers who left their jobs voluntarily and hence are ineligible for the UI, was low during the recent recession, and usually the job leavers are composed by way much more people who have better job opportunities than job losers who got laid off but are eligible for the UI. Therefore, this strategy also can overstate the effect of UI benefits. In this specification, the distinct effects on those unemployed more or less than 26 weeks are isolated as well, and the negative effects are concentrated among individuals unemployed for more than 26 weeks.

The third strategy only focuses on the variation in EB, which purely replies on whether states participate in the programs when the state unemployment rates reach the trigger. Since the paper assumes that individuals do not anticipate future legislative changes of the EUC benefits, by isolating the effects of EB, this strategy can remove the influences of the assumption to the estimates. Moreover, the remaining variations should mainly come from states with similar economic situations. Besides, under these three strategies, Rothstein (2011) also uses multinomial logit model to explore the distinction between reemployment and labor force exit. Benefit extensions appear to lead to larger reductions in the probability of labor force exit than in the probability of reemployment.

The last strategy investigates the interaction between the number of available benefit weeks and the number of weeks that the individuals have used up to date.

$$ln(\frac{\lambda_{ist}}{1-\lambda_{ist}}) = f(d_{ist};\theta) + \gamma_v \sum_{v=0}^{99} 1(n_{ist} = v) + \alpha_{st}, \tag{11}$$

where d_{its} represents the number of weeks of benefits remaining, with a flexible function f. The summation term is a full set of indicators for unemployment durations ranging from 0 to 99 weeks. As indicated in the job search model, this strategy captures the stronger effects of the extension of UI benefit on individuals who will exhaust the benefits immediately and only get to add the extension to the end, than on individuals who anticipate the extension at the beginning of the UI.

According to Rothstein (2011)'s results, the availability of extended UI benefits caused small reductions in the probability that unemployed workers exited unemployment, reducing the monthly hazard in the fourth quarter of 2010 by between one and three percentage points on a base of 22.4%. Not more than half of the unemployment exit effect comes from effects on reemployment: one of the specifications indicates that UI extensions reduced the average monthly reemployment hazard of unemployed displaced workers in 2010:Q4 by 0.5 percentage points (on a base of 13.4%), and reduced the monthly labor force exit hazard by 1.0 percentage points (on a base of 9.0%). Even the specification yielding the largest effects indicates that UI extensions contributed only 0.5 percentage points to the unemployment rate. Moreover, simulations that include only the labor force participation effects yield estimates at least half as large as do simulations with both participation and reemployment effects, suggesting that reduced job search due to UI extensions raised the unemployment rate by only 0.1 to 0.2 percentage points.

Farber and Valletta (2013) uses the rotation group of the Current Population Survey from January 2000 to December 2005 (2000m1-2005m12) and from January 2007 to December 2012 (2007m1-2012m12) to examine the effects of the two episodes of extended benefits in the 2000s.

Using the matched individuals for four consecutive months produces a sample of longer unemployment spells than the actual ones, and hence Farber and Valletta (2013) adopts a discrete-time hazard specification to model the conditional probability that the unemployment spell ends at certain duration. Assuming a standard normal distribution for the hazard probability, the corresponding probit model has monthly observations on an unemployment spell matched to the succeeding month. The paper also applies a competing risk model to analyze the transition out of unemployment to new jobs, and exiting unemployment to leave the labor force.

To assess the impact of UI extensions, Farber and Valletta (2013) also compiled a detailed database of trigger dates and maximum available UI weeks at the state level. The extension of UI availability proceeded gradually, and its extent and timing varied across states. Instead of including simulated available benefit weeks between the time of displacement and the initial CPS interviews, this paper uses two dummy variables to capture the gradual extensions of UI: the indicator for availability of extended benefits, EB_{it}, and the indicator for whether individual i is in the last month of availability of benefits, $last_{it}$.

$$y_{it} = \beta X_{it} + \delta_1 EB_{it} + \delta_2 Last_{it} + \varepsilon_{it}, \tag{12}$$

where the unobserved latent variable y_{it} is positive if a spell ends in a given month. X vector includes a set of standard personal characteristics and economic variables: a cubic in the monthly seasonally adjusted state unemployment rate and a cubic in the 3-month annualized growth in seasonally adjusted log non-farm payroll employment. The model also includes a set of indicators for the unemployment durations. A complete set of date (year-month) and state indicators, which provide additional controls for relative economic conditions, are also included.

Expected effect of extended UI for UI-ineligible group should be smaller than for UI eligible individuals. However, since the variables used in the model do not account for the effects of the variation in economic conditions across states over time, which is correlated with the extension of the UI, the estimates of the effect of extended UI on the UI-eligible group could be overstated. From the other side, the spillover effects of extended UI on the job finding for the UI-ineligible group can be implied as well.

To confirm the fact that based on the March CPS that only a small fraction of job leavers and new entrants report having received UI, Farber and Valletta (2013) also does a placebo test which reestimate the probit model of exit from unemployment on samples of UI ineligible unemployed individuals. In principal, the UI-ineligible group should be largely unaffected by the availability of extended benefits, and hence if the classification of UI-eligible and UI-ineligible groups is correct, the re-estimated results should not be the same as before. If the re-estimated results have the same negative signs as on the UI-eligible group, then the specification may not fully controlled the state/month specific economic conditions and the estimates of the results on the UI-eligible group can be overestimated the real effects. If the re-estimated results have the opposite positive signs as on the UI-eligible group, i.e. a positive effect of extended benefits on exit from unemployment among the UI-ineligible group, then this indicates that extended benefits reduce the job finding rate among the UI-eligible while improving job opportunities for the UI-ineligible. This is called positive externality in the old literature. The placebo test shows that there is no effect of the extension on the UI-ineligible group in the earlier period while in the later period the availability of extended benefits is correlated with unmeasured economic conditions.

Small but statistically significant effects of UI are found by Farber and Valletta (2013): reduce the unemployment exits, increase the unemployment durations and the estimated expected unemployment durations as well. During the recession, extended benefits increased the expected duration of unemployment by about 7 percent but accounted for about 22 percent of long-term unemployment in the cross-section. Yet this founding arises the conflict, because extended benefits have only small effects on the distribution of unemployment duration early in spells($\geq$26 weeks), and the substantial majority of spells end in the first 26 weeks. This paper also finds that the effect on exit from unemployment occurs primarily through a reduction in labor force exits rather than through exit to employment, which implies that extended benefits do not delay the time to re-employment substantially and the major effect of extended benefits is providing income to job losers who would have exited the labor force otherwise. Overall, this paper estimate that extended UI increased the whole unemployment rate by only about 0.4 percentage points in the recent recession, which is small in comparison with the peak unemployment rate of 10 percent.

Fujita (2011) uses the monthly Current Population Survey (CPS) data to estimate the transition rates from unemployment to employment (UE) and from unemployment to leaving labor force (UN). This paper only focuses on male workers as in Moffitt (1985), Meyer (1990) and Katz and Meyer (1990) given that women are often secondary earners in a household, which may complicate the interpretation of the results. This paper distinguishes the exit rates between job finding and dropping out of the labor force. The CPS data is used for the Bureau of Labor Statistics (BLS) official labor market statistics rates such as the unemployment rate, and hence it is easily to be translated the effects on transition rates into the unemployment rate. The sample covers the period between January 2004 and July 2010, excluding the observations for 2008. The data prior to 2008 are used to infer the shape of the hazard functions when no UI benefits are available beyond 26 weeks, whereas the post-2008 data are used to estimate the hazard functions during the period of large extensions. Since the first extension in response to the Great Recession is introduced in the middle of 2008, by excluding the 2008 data, it is easily to contrast the shapes of the hazard functions between the period with and without extensions.

Fujita (2011) uses a multinomial logit regression model. Let S_i be the labor market status in $t+!$ of an unemployed individual i in t The probability of a transition into s in $t+1$ is modeled by:

$$\Pr(S_i) = \frac{exp(y_i)}{\sum\limits_{s} exp(y_i)}, \tag{13}$$

where function y_i is defined as follow:

$$y_i = \alpha_s X_i + \beta_s D_i + EB\Delta_s D_i + \eta_s v_t \tag{14}$$

The vector X_i represents time-invariant individual characteristics. The variable D_i is a vector of dummy variables, indicating which unemployment duration bin the worker is in. A dummy variable EB takes 1 when extended benefits are available and 0 otherwise. In other words, if a worker is unemployed during the 2009-2010 period, then EB=1 and 0 otherwise. v_t represents the job vacancy rate in month t measured in the JOLTS, and this variable serves to control for the differences in the business cycle conditions. The assumption that implied in this model is that job vacancies are exogenous to other variables, which is not true because the number of job seekers (which is affected by the generosity of benefits) influences the vacancy creation. Therefore, the estimated effect of the UI extension on the hazard rates is smaller than the actual one. Fujita (2011) finds that UI benefit extensions have raised the male unemployment rate by around 1.2 percentage points.

Appendix B Proofs of the model in Rothstein (2011)

An unemployed individual's income is y_0 if she does not receive UI benefits and $y_0 + b$ otherwise. Her utility is $u(c) - s$, where c is the consumption and s is the amount of effort she devotes to research. If she finds a job, the utility becomes $u(w)$, where w is wage assumed as exogenous permanent. The probability that she finds a job is a increasing function of search effort, $p(s)$, with $p^{'}(s) > 0$, $p^{''}(s) < 0$, $p(0) = 0$, $p^{'}(0) = \infty$, and $p(s) < 1$ for all s. Unemployment benefits are available for up to D periods of unemployment. These assumptions lead to a dynamic decision problem corresponding to the number of weeks of benefits remaining. Letting $V_U(d)$ represent the value function of an unemployed individual with $d > 0$ weeks of benefits remaining, V_E represents the value function of an employed worker, the Bellman equation is

$$V_U(d) = \max_{s_d} u(y_0 + b) - s_d + \delta[p(s_d)V_E + (1 - p(s_d))V_U(d - 1)], \tag{15}$$

where s_d represents the chosen search effort, and δ is the per-week discount rate. Once benefits are exhausted ($d = 0$), the problem becomes stationary:

$$V_U(0) = \max_{s_0} u(y_0) - s_0 + \delta[p(s_0)V_E + (1 - p(s_0))V_U(0)], \tag{16}$$

The first order condition of equation (1) then implies that the search effort choice satisfies:

$$p'(s_d) = \frac{1}{\delta(V_E - V_U(d - 1))} \tag{17}$$

for $d \geq 1$. From equation (34), two main results are found.

Proposition 1: The value function $V_U(d)$ is increasing in d

From equation (32) and (33), we know that the maximization problem is identical when $d = 1$ as when $d = 0$. The equation (31) becomes:

$$p'(s_0) = p'(s_1) = \frac{1}{\delta(V_E - V_U(0))} \tag{18}$$

Thus, $s_0 = s_1$ and $V_U(1) > V_U(0)$. The optimal s is denoted as s_d given d weeks of benefits remaining. Assume that $V_u(x) > V_u(x-1)$ for some $x > 0$, then

$$\begin{aligned} V_U(x+1) - V_U(x) &= V_U(s_{x+1}, x+1) - V_U(s_x, x) \\ &\geq V_U(s_x, x+1) - V_U(s_x, x) \\ &= \delta(1-p(s_x))(V_u(x) - V_u(x-1)) \end{aligned} \tag{19}$$

Hence, $V_U(d+1) > V_U(d)$ for all d, and the first proposition can be proved

Proposition 2: Search effort increases as exhaustion approaches, reaching its final level in the second to the last period of benefit receipt: $s_{d+1} < s_d < s_1 = s_0$ for all $d \geq 2$.

Equation (34) implies that the $p'(s_{d+1}) > p'(s_d)$ when $V_U(d+1) > V_U(d)$. Since $p'' < 0$ as assumed, $s_{d+1} < s_d$.

The first two propositions show that as approaching to the UI benefit expiration, search intensity rises, and individuals who are running out of their benefits exert higher search efforts than individuals who are at the beginning of their unemployment spell and still having a lot of benefit available. Hence, the extension of the insurance benefit will reduce the search efforts for people who would have invested more efforts on job searching as approaching to the end of the benefits, and reduce the probability of exiting from UI to jobs. The magnitude of this reduction effects on individuals depends of the shape of the $p()$ function.

Besides, the paper assumes that only if unemployed workers continue job search and exert search effort higher than certain given level $\theta > 0$, they will receive the same amount insurance benefit across limited time. For those who exert lower search effort, they receive 0 benefit payment, but preserve their benefit entitlements in the future. The Bellman equation becomes:

$$\tilde{V}_U(d) = \max_{s_d} \begin{cases} u(y_0+b) - s + \delta[p(s)V_E + (1-p(s))\tilde{V}_U(d-1)] & \text{if } s \geq \theta \\ u(y_0) - s + \delta[p(s)V_E + (1-p(s))\tilde{V}_U(d)] & \text{if } s < \theta. \end{cases} \tag{20}$$

The second main finding based on the assumption above is that the extension of the unemployment benefit continues the unemployed workers search intensity, which could potentially increase the probability of finding a job. Combining the two findings, the net effect on the probability of exiting from UI to jobs is ambiguous.

Proposition 3: Any individual who chooses search effort $s \geq \theta$ with d weeks of benefits remaining would also choose $s \geq \theta$ with d' weeks remaining, for all d, $d' > 0$

In order to prove Proposition 3, Rothstein (2011) lets η to indicate whether the optimal search effort $\tilde{s}_d$ is bigger than the threshold θ at d, $\eta_d = \eta_{d-1} = ... = \eta_0$, and $\eta_{d+1} \neq \eta_d$ for any $d > 0$, and shows contradictions by this setting in two cases. The first case is when $\eta_d = 1(\tilde{s}_d \geq \theta)$, so the individual receives the benefit for all $x \leq d$. Thus $\tilde{s}_1 = \tilde{s}_0$, $\tilde{s}_{x+1} < \tilde{s}_x$, and $\tilde{V}_{x+1} > \tilde{V}_x$ for all $x > 0$. By the assumption $\eta_{d+1} \neq \eta_d$, the η_{d+1} must be equal to 0, and the $\tilde{s}_{x+1} < \theta$. Then equation (37) becomes

$$\begin{aligned}\tilde{V}_U(d+1) &= \max_{s_{d+1}<\theta} u(y_0) - s_{d+1} + \delta[p(s_{d+1})V_E + (1 - p(s_{d+1}))\tilde{V}_U(d+1)] \\ &= \max_{s_{d+1}<\theta} \frac{u(y_0) - s_{d+1} + \delta p(s_{d+1})V_E}{1 - \delta[1 - p(s_{d+1})]} \qquad (21)\end{aligned}$$

$$\tilde{V}_U(d) = \max_{s_d \geq \theta} u(y_0 + b) - s_d + \delta[p(s_d)V_E + (1 - p(s_d))\tilde{V}_U(d-1)] \qquad (22)$$

Equation (38) implies that the right-hand side does not vary with the period. In another word, $\tilde{V}_U(s_{d+1}, d+1) = \tilde{V}_U(s_{d+1}, d)$. However, $\tilde{V}_U(s_{d+1}, d+1) > \tilde{V}_U(s_d, d) > \tilde{V}_U(s_{d+1}, d)$ yields a contradiction.

The second case when $\eta_d = \eta_{d-1} = ... = \eta_0 = 0$, but $\eta_{d+1} = 1$. So the individual receives the benefit only at $d+1$. Since $\tilde{s}_{d+1} < \tilde{s}_d$, and $\tilde{s}_{d+1} > \theta$, the $\tilde{s}_d$ must be bigger than θ, which contradicts to the assumption too.

Appendix C Other discrete-time hazard analysis

Other than the logit model, in this section I present alternative duration models to check the robustness of results from logit models. The first hazard model I use is the complementary log-log model [16]. Unlike the logit model, which assumes that the hazard rate is symmetric around 0.5, the complementary log-log model fits the data which do not support symmetric exit rates in the $[0, 1]$ interval, but have the rates increasing sharply at small to moderate value but increases slowly near 1. Relative to the assumption that the hazard rates have a logistic distribution, the assumption of the hazard rate distribution of the complementary log-log model is more suitable for the UI study because the probability of leaving unemployment occurs much less frequently than the event of ending benefit weeks without being covered by employment. The complementary log-log has the form:

$$log(-log(1 - \lambda(X_{it}))) = X_{it}^{'}\beta, \tag{23}$$

where $\lambda(X_{it})$ is the exit hazard for jobs, and X_{it} include all the regressors I used in the specification (1.1) and with the same form.

The other approach I used to exam the effects of extension is Cox-proportional hazard model. The time at risk has a range from $t = 0$ (zero benefit weeks) to $t = 99$ (the maximum 99 benefit weeks). The hazard rate function for each observation i at time t is assumed to take the form:

$$h_i(t) = \lambda_0(t) \exp(X_i^{'}(t)\beta), \tag{24}$$

where $X_i(t)$ is a vector of covariates summarizing observed differences between individuals at t. $\lambda_0(t)$ is the baseline hazard function which are the time intervals at risk of the event (exiting for jobs) occurring for each person, and may take a parametric or non-parametric form. In this study, I use a non-parametric form for the baseline hazard function by defining 100 dummy variables, and each one represents the number of benefit weeks at risk of leaving UI for new jobs.

The interpretation of the coefficients can be easily translated into the elasticities of exit hazards with respect to other regressors, and this will facilitate me to compare my results with the previous literature [17]. One advantage of generic proportional hazard model is that the base-

[16] See Powers and Xie (2000)

[17] When the exist hazards are very small, both of the estimates of the complementary log-log model and the logit

line hazard function can be estimated non-parametrically, as I used in the logit model. Different specifications of the baseline hazard function can vary the estimates largely. Non-parametrically estimating the baseline hazard avoids inconsistent estimation of covariate coefficients due to a mis-specified baseline hazard [18]. The main advantage of Cox-proportional model is that the baseline hazard has "canceled out" in the partial likelihood shown below, and hence the specification of baseline hazards does not affect the estimates.

To simplify, all intervals are assumed to be of unit length, and hence the recorded duration for each person i corresponds to the interval $[t_i - 1, t_i)$. Given the proportional hazard assumption, the probability that an unemployment spell does not end before time t_{i-1} can be represented as the survivor function, which at the time interval $[t_i - 1, t_i)$ has the form:

$$\Pr(T \geq t_i - 1) = S(t_i - 1; X_{it}) = exp\{-exp[(X_{it}^{'}\beta) + ln(\Lambda_{t_i - 1})]\}, \tag{25}$$

where $\Lambda_{t_i-1} = \sum_{s=0}^{t_i} h_s$. The corresponding discrete time hazard in the interval, which describes the probability that a spell ends between $t_i - 1$ and t_i, given that spell has survived to $t_i - 1$:

$$\begin{aligned} h_j(X_{it}) &= \Pr(T \in [t_i - 1, t_i | T \geq t_i - 1) = 1 - \frac{S(t_i; X_{it})}{S(t_i - 1; X_{it})} \\ &= 1 - exp\{-exp[(X_{it}^{'}\beta) + ln(\Lambda_{t_i-1,t_i})]\}, \end{aligned} \tag{26}$$

where $\Lambda_{t_i-1,t_i} = \sum\limits_{s=t_i-1}^{t_i} h_s$.

In my study, the binary dependent variable is whether UI claimants leaving UI for jobs $y_i = 1$, or otherwise $y_i = 0$. The former group contributes completed spell data, while the latter group contributes right-censored spell data. With these assumptions, the log-likelihood $logL(\beta, \Lambda)$ can be written as:

$$\begin{aligned} logL(\beta, \Lambda) &= \sum_{i=1}^{n}\{y_i \ln\left[S(t_i - 1; X_{it})h_{t_i}(X_{it})\right] + (1 - y_i)\ln\left[S(t_i; X_{it})\right]\} \\ &= \sum_{i=1}^{n}\{y_i \ln\left[h_{t_i}(X_{it})\prod_{s=1}^{t_i-1}(1 - h_s(X_{it}))\right] + (1 - y_i)\sum_{s=1}^{t_i}(1 - y_i)\ln\left[1 - h_s(X_{it})\right]\} \\ &= \sum_{i=1}^{n}\{y_i \ln\left[\frac{h_t(X_{it})}{1 - h_t(X_{it})}\right] + \sum_{s=1}^{t_i}\ln\left[1 - h_s(X_{it})\right]\} \end{aligned} \tag{27}$$

model can also be interpreted as the elasticity of exit hazards with respect to other regressor.

[18] Due to the limited computational time, I only use 20% of my data to get results from the nonparametric analysis.

Maximizing this log-likelihood with respect to β by plugging the equation (24) in, I can get the estimate β, and the baseline hazard cancels out. This log likelihood function (27) can be written as a function of a binary dependent variable y_{it} with value of 1 if individual i ends benefit spell with a job covered, and 0 otherwise.

One main concern in the hazard analysis is the unobserved heterogeneity that induces an underestimate of the extent to which the exit hazard rate increases with duration. (i.e. over-estimate of the decline in the exit hazards as the unemployment duration raising), and attenuates the magnitude of the impact of covariates on the hazard rates [19]. A common way to capture the unobserved heterogeneity between individuals is to incorporate a Gamma distributed random variable. The proportional hazard is now specified as:

$$h_{it} = \lambda_0(t)\varepsilon_i exp(X_{it}^{'}\beta) = \lambda_0(t)exp(X_{it}'\beta + \ln(\varepsilon_i)) \tag{28}$$

where ε_i is a Gamma distributed random variable with unit mean and variance σ^2. The discrete-time hazard function is now:

$$h_{it} = 1 - exp\{-exp[(X_{it}'\beta) + \ln(\Lambda_t) + \ln(\varepsilon_i)]\} \tag{29}$$

Alternatively, the unobserved heterogeneity can also be treated non-parametrically. Heckman and Singer (1984) develop a consistent nonparametric maximum likelihood estimator for the distribution of unobservables, by allowing for an arbitrary distribution for the individual heterogeneity term. This is reflected in the hazard function incorporating an extra term allowing for different intercepts for people with different unobserved characteristics. In my specification, I assume that there are two general types (type=1, 2) people including in the data, then the hazard will be

$$h_{it} = 1 - exp\{-exp[m_j + X_{it}^{'}\beta + \ln(h_t)]\}, j \in 1, 2 \tag{30}$$

where m_j represents the discrete points of a bivariate variable. The probability that an individual is of of type 1 (2) is P_1 ($P_2 = 1 - P_1$), and contribute m_1 (m_2) to the hazard rates. The predicted

[19] Another reason to choose the non-parametric form of the baseline hazard function is that parametric models which tightly constrain the general shape of the baseline hazard, for instance Weibull, when in fact the pattern of data may be non-monotonic, and hence could bias the estimates of the duration dependence. Moreover it is well-known that "the significance of unobserved heterogeneity are more reliably drawn if a flexible specification for the baseline hazard has been used".

exit hazard for each individual i at time t can be estimated by:

$$\widehat{h_i(t)} = \widehat{\lambda_0(t)} exp(X'_{it}\hat{\beta}) \tag{31}$$

where the baseline hazard can be estimated given by Cox and Oakes (1984, Section 7.8):

$$\widehat{h_0(t)} = \frac{1}{\sum\limits_{i,t,e_{it}} exp(X'_{it}\hat{\beta})} \tag{32}$$

Table 5 presents the estimated results for the complementary log-log model without considering any potential unobserved heterogeneity, for the Cox - proportional hazard model treating unobserved heterogeneity non-parametrically, for the Cox - proportional hazard model assuming a Gamma distribution for unobserved heterogeneity. In both of the proportional hazard models, I treat the baseline hazard non-parametrically by creating 100 interval-specific dummy variables (one for each spell week at risk), as the longest observed spell in my data set is 99 weeks and the shortest benefit week is 0.

Comparing across the three models, it can be observed that the three estimates give quite similar results. For the non-parametric model, I assume that there are two types people. As indicated in the results, the probability of being type 1 is insignificantly different from the probability of being type 2. (i.e. P-value of the difference of being two different people is 0.802) According to this, I fail to reject the null hypothesis that the type 1 is no different from the type 2, and can conclude that there is no unobserved individual heterogeneity. For the model assuming the Gamma distribution, the null hypothesis is that unobserved heterogeneity variance component is equal to zero, namely the unobserved heterogeneity does not exist. According to the test statistics, I fail to reject the null hypothesis, and hence I conclude that there is no heterogeneity issue in this model as well.

As indicated in Table 5, the significant negative effects of the indicator for whether individuals are at their last week of UI benefit show that individuals who have more than one benefit weeks available have much lower possibilities to exit UI for jobs. As the remaining benefit weeks increasing, the probability of exiting UI decreases, yet the effects are insignificant. The base group is defined as non-white female who are younger than 22 years old, high school drop outs, and used to work in agriculture sector. The coefficient estimate gives the change in the exit hazards when a discrete change from a base category occurs. For example, relative to other races, white workers have lower

Table 5: Use Alternative Hazard Models to estimate the effect of extensions to UI duration

Variables	Complementary		Cox Proportional Hazard			
	log-log		Non-para Heter		Gamma Dist. Heter	
	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
I(Remaining benefit>0)	-0.56664	0.05530	-0.58738	0.05735	-0.59539	0.05712
# Remaining benefit wks	-0.00063	0.00127	-0.00103	0.00131	-0.00116	0.00131
male	0.11687	0.01850	0.12267	0.01970	0.12464	0.02023
white	-0.04022	0.02503	-0.04216	0.02641	-0.04549	0.02722
Age						
22-31	-0.28114	0.02711	-0.29741	-0.03062	-0.30566	0.03133
32-41	-0.36054	0.02828	-0.38362	0.03366	-0.39547	0.03425
42-51	-0.47202	0.02871	-0.49996	0.03557	-0.51480	0.03622
52-62	-0.73102	0.03416	-0.76824	0.04329	-0.78748	0.04401
>63	-1.19689	0.06632	-1.25070	0.07678	-1.27908	0.07811
Years of Education						
9	0.04709	0.02000	0.04651	0.02098	0.04534	0.02157
12	-0.01805	0.05474	-0.02642	0.05744	-0.03070	0.05900
16	0.05510	0.04354	0.05521	0.04559	0.05539	0.04690
20	0.05499	0.02621	0.05457	0.02745	0.05360	0.02823

Variables	Complementary log-log		Cox Proportional Hazard			
			Non-para Heter		Gamma Dist. Heter	
	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
Industry						
Mining	0.19515	0.07572	0.20093	0.07928	0.20559	0.08154
Utilities	-0.18839	0.37993	-0.22579	0.39478	-0.21980	0.40257
Construction	0.34499	0.04347	0.34924	0.04543	0.35217	0.04670
Manufacturing	0.36557	0.04211	0.38204	0.04530	0.39045	0.04639
Wholesale Trade	0.10857	0.05594	0.10476	0.05838	0.10423	0.05994
Retail Trade	-0.01056	0.04648	-0.01612	0.04854	-0.01768	0.04979
Transportation and Warehousing	0.25999	0.05093	0.26812	0.05354	0.27320	0.05509
Information	-0.00455	0.07840	-0.01339	0.08176	-0.01647	0.08387
Finance and Insurance	-0.01022	0.06850	-0.02101	0.07155	-0.02551	0.07335
Real Estate/Rental and Leasing	0.00026	0.08388	-0.00652	0.08729	-0.00873	0.08957
Professional, Scientific, and Technical Services	0.10832	0.05256	0.10031	0.05501	0.09768	0.05639
Management of Companies and Enterprises	0.15759	0.14321	0.16285	0.14990	0.16670	0.15399
Administrative-Support	0.20568	0.04019	0.21044	0.04203	0.21427	0.04322
Educational Services	-0.03642	0.12015	-0.04354	0.12469	-0.04657	0.12788
Health Care	0.23830	0.04650	0.24533	0.04877	0.25040	0.05020
Arts-Entertainment	0.29399	0.07173	0.30529	0.07555	0.31133	0.07770
Accom.-Food Services	0.25678	0.04241	0.26769	0.04497	0.27436	0.04623
Other Services	0.19897	0.05003	0.20209	0.05227	0.20506	0.05376
Public Administration	0.05365	0.19586	0.05303	0.20381	0.05362	0.20927
Constant	-16.79126	527.06250	-16.72912	0.19121	-16.56403	0.22206
# of Claims	117,504		117,504		117,504	
			P(type 1)=.3481642		LR test of Gamma var=0	
			P(type 2)=.6518358		chibar2(01)=5.58312	
			P-value(diff)=0.802		Prob.>=chibar2=0.9067	

Note: In these specifications, I control for the individual-level covariates, fixed time effects for calendar month, three-order polynomial insured unemployment rates, three-order polynomial new UI claims rates, and baseline hazard.

hazard rates according to the negative estimates from three models. Male unemployed individuals have higher exit hazards than female benefit claimants. With respect to educational attainment, more education often associated with higher job finding rates: relative to individuals whose education levels are lower than high school, more educated workers are more likely to reemployed, and have significant positive estimates. Relative to individuals who are less than 252 people with higher ages are more difficult to leave UI for jobs. This result may reflect the differences in the type of jobs typically associated with each age group, but this finding is also consistent with the findings from previous literature showing that younger workers have higher labor turnover in general, which means the problem of jobs for the young is not that they are hard to find, but that they do not last very long. The findings are quite consistent with the results of the logit model.

From the predicted hazard demonstrated in Table 6, we can see the diminishing exit hazards as individuals unemployment duration increasing. The highest predicted probability of leaving UI for jobs happens at the end of the state UI, i.e. with benefit weeks of 26 weeks, and longer unemployment durations reduce the possibilities of finding jobs. But when people are about to exhaust their total benefit and getting closer to the end of 99 benefit weeks, the predicted exiting hazards raise.

Table 6: Predicted Hazard Rates From Survival Models

	(1) log-log		(2) Cox- nonpara		(3) Cox - Gamma Distr			(1) log-log		(2) Cox- nonpara		(3) Cox - Gamma Distr	
	Hazard	SE	Hazard	SE	Hazard	SE		Hazard	SE	Hazard	SE	Hazard	SE
0 wk	0.27996	0.00000	0.18736	0.00000	0.29104	0.00000	50 wk	0.02346	0.25689	0.02406	0.27303	0.02830	0.27894
1 wk	0.07669	0.00289	0.05195	0.05110	0.08106	0.01512	51 wk	0.00798	0.23160	0.00820	0.21959	0.00966	0.20835
2 wk	0.12027	0.00002	0.08430	0.00287	0.12762	0.00035	52 wk	0.03486	0.13357	0.03582	0.15378	0.04211	0.16606
3 wk	0.06397	0.01050	0.04552	0.08230	0.06835	0.03661	53 wk	0.01382	0.31211	0.01423	0.30912	0.01675	0.30152
4 wk	0.12908	0.00001	0.09521	0.00096	0.13831	0.00013	54 wk	0.01402	0.31342	0.01444	0.31092	0.01700	0.30352
5 wk	0.06066	0.01447	0.04542	0.08225	0.06553	0.04380	55 wk	0.01002	0.27634	0.01032	0.26671	0.01216	0.25547
6 wk	0.10455	0.00013	0.08043	0.00410	0.11324	0.00118	56 wk	0.03160	0.16694	0.03259	0.18634	0.03830	0.19825
7 wk	0.05331	0.02870	0.04155	0.10717	0.05812	0.06963	57 wk	0.02055	0.28942	0.02123	0.29984	0.02498	0.30208
8 wk	0.08058	0.00192	0.06402	0.01831	0.08805	0.00875	58 wk	0.02506	0.24172	0.02592	0.25756	0.03048	0.26532
9 wk	0.05139	0.03409	0.04134	0.10819	0.05644	0.07671	59 wk	0.02064	0.28876	0.02137	0.29894	0.02514	0.30131
10 wk	0.09290	0.00050	0.07627	0.00600	0.10219	0.00288	60 wk	0.01534	0.31919	0.01590	0.31970	0.01871	0.31402
11 wk	0.04961	0.03989	0.04120	0.10864	0.05491	0.08360	61 wk	0.00940	0.27026	0.00975	0.26096	0.01149	0.24924
12 wk	0.07468	0.00357	0.06298	0.01984	0.08281	0.01283	62 wk	0.01263	0.31206	0.01310	0.30737	0.01543	0.29813
13 wk	0.05130	0.03444	0.04373	0.09093	0.05715	0.07288	63 wk	0.00808	0.24376	0.00839	0.23346	0.00988	0.22162
14 wk	0.05980	0.01577	0.05157	0.05065	0.06678	0.03946	64 wk	0.02732	0.21710	0.02837	0.23337	0.03337	0.24297
15 wk	0.04910	0.04176	0.04275	0.09701	0.05503	0.08247	65 wk	0.02090	0.28915	0.02175	0.29891	0.02560	0.30157
16 wk	0.06485	0.00967	0.05711	0.03232	0.07282	0.02610	66 wk	0.01148	0.30361	0.01196	0.29760	0.01408	0.28704
17 wk	0.04239	0.07305	0.03763	0.13556	0.04779	0.12336	67 wk	0.01164	0.30618	0.01213	0.30042	0.01429	0.28998
18 wk	0.07738	0.00270	0.06954	0.01097	0.08731	0.00908	68 wk	0.01843	0.31117	0.01919	0.31710	0.02259	0.31620

	(1)		(2)		(3)			(1)		(2)		(3)	
	log-log		Cox- nonpara		Cox - Gamma Distr			log-log		Cox- nonpara		Cox - Gamma Distr	
19 wk	0.04480	0.06019	0.04061	0.11155	0.05083	0.10435	69 wk	0.01553	0.32460	0.01617	0.32547	0.01904	0.32008
20 wk	0.06733	0.00758	0.06164	0.02192	0.07650	0.02002	70 wk	0.00706	0.22189	0.00736	0.21223	0.00866	0.20043
21 wk	0.04972	0.03971	0.04589	0.07691	0.05674	0.07390	71 wk	0.01952	0.30525	0.02036	0.31270	0.02396	0.31355
22 wk	0.07082	0.00533	0.06599	0.01496	0.08094	0.01449	72 wk	0.01807	0.31678	0.01887	0.32191	0.02221	0.32055
23 wk	0.04769	0.04740	0.04476	0.08330	0.05477	0.08295	73 wk	0.01294	0.32275	0.01352	0.31924	0.01591	0.31011
24 wk	0.06777	0.00727	0.06412	0.01759	0.07793	0.01798	74 wk	0.01486	0.32874	0.01552	0.32847	0.01827	0.32203
25 wk	0.04816	0.04559	0.04586	0.07677	0.05563	0.07869	75 wk	0.01883	0.31329	0.01969	0.31944	0.02317	0.31932
26 wk	0.27133	0.00000	0.26484	0.00000	0.30948	0.00000	76 wk	0.00775	0.24583	0.00811	0.23683	0.00955	0.22449
27 wk	0.01209	0.28533	0.01198	0.27461	0.01427	0.26748	77 wk	0.01366	0.32929	0.01430	0.32710	0.01684	0.31897
28 wk	0.03233	0.15493	0.03209	0.18733	0.03811	0.19610	78 wk	0.01777	0.32282	0.01863	0.32722	0.02193	0.32544
29 wk	0.01338	0.29594	0.01330	0.28931	0.01581	0.28288	79 wk	0.11139	0.00006	0.11673	0.00010	0.13622	0.00015
30 wk	0.02594	0.22401	0.02583	0.25110	0.03066	0.25716	80 wk	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
31 wk	0.01368	0.29821	0.01364	0.29272	0.01620	0.28639	81 wk	0.01475	0.34602	0.01556	0.34590	0.01836	0.33937
32 wk	0.02365	0.24918	0.02363	0.27167	0.02801	0.27615	82 wk	0.00503	0.17666	0.00531	0.16952	0.00622	0.15800
33 wk	0.01807	0.29614	0.01807	0.30443	0.02143	0.30283	83 wk	0.01520	0.34777	0.01605	0.34826	0.01896	0.34249
34 wk	0.02329	0.25340	0.02333	0.27458	0.02764	0.27889	84 wk	0.01036	0.31971	0.01095	0.31421	0.01294	0.30257
35 wk	0.02054	0.27976	0.02061	0.29455	0.02440	0.29598	85 wk	0.01800	0.33585	0.01903	0.33994	0.02250	0.33842
36 wk	0.02288	0.25812	0.02300	0.27773	0.02721	0.28190	86 wk	0.01039	0.32038	0.01099	0.31513	0.01300	0.30367
37 wk	0.01582	0.30453	0.01593	0.30592	0.01884	0.30148	87 wk	0.01053	0.32347	0.01114	0.31838	0.01318	0.30716
38 wk	0.02778	0.20498	0.02802	0.23029	0.03309	0.23898	88 wk	0.01609	0.34939	0.01704	0.35110	0.02016	0.34673
39 wk	0.01785	0.29843	0.01805	0.30542	0.02133	0.30349	89 wk	0.02988	0.19845	0.03166	0.20909	0.03744	0.21900
40 wk	0.03273	0.15227	0.03318	0.17754	0.03912	0.18858	90 wk	0.01404	0.35539	0.01490	0.35468	0.01764	0.34737
41 wk	0.02237	0.26477	0.02271	0.28140	0.02679	0.28544	91 wk	0.01730	0.34818	0.01837	0.35122	0.02175	0.34872
42 wk	0.02261	0.26259	0.02299	0.27936	0.02710	0.28377	92 wk	0.01458	0.35874	0.01549	0.35872	0.01835	0.35221
43 wk	0.00810	0.22837	0.00825	0.21513	0.00973	0.20452	93 wk	0.01783	0.34652	0.01895	0.35008	0.02246	0.34837
44 wk	0.03027	0.17859	0.03084	0.20195	0.03630	0.21284	94 wk	0.00617	0.23149	0.00657	0.22553	0.00772	0.21245
45 wk	0.01565	0.30917	0.01596	0.31020	0.01881	0.30521	95 wk	0.01555	0.36234	0.01652	0.36342	0.01958	0.35837
46 wk	0.03410	0.13989	0.03482	0.16249	0.04093	0.17469	96 wk	0.00327	0.11924	0.00348	0.11486	0.00412	0.10799
47 wk	0.00636	0.18610	0.00650	0.17341	0.00766	0.16335	97 wk	0.02046	0.33025	0.02175	0.33614	0.02578	0.33825
48 wk	0.03157	0.16571	0.03228	0.18803	0.03796	0.19971	98 wk	0.03540	0.14177	0.03762	0.15104	0.04459	0.16110
49 wk	0.01294	0.30475	0.01326	0.29955	0.01562	0.29096	99 wk	0.14340	0.00000	0.15136	0.00000	0.17756	0.00000

Bibliography

Card, D., Johnston, A., Leung, P., Mas, A., and Pei, Z. (2015). The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression kink design in missouri, 2003-2013. *American Economic Review*, 105:126–30.

Card, D., Lee, D., Pei, Z., and Weber, A. (2012). Nonlinear policy rules and the identification and estimation of causal effects in a generalized regression kink design. *NBER Working Papers*.

Card, D. and Levine, P. B. (2000). Extended benefits and the duration of ui spells: evidence from the new jersey extended benefit program. *Journal of Public Economics*, 78:107–138.

Chetty, R. (2008). Moral hazard versus liquidity and optimal unemployment insurance. *Journal of Political Economy*, 116:173–234.

Fan, J. and Gijbels, I. (1992). Variable bandwidth and local linear regression smoothers. *The Annals of Statistics*, 20(4):2008–2036.

Farber, H. S. and Valletta, R. G. (2013). Do extended unemployment benefits lengthen unemployment spells? evidence from recent cycles in the u.s. labor market. *NBER Working Papers*.

Florens, J.-P., Heckman, J. J., Meghir, C., and Vytlacil, E. J. (2008). Identification of treatment effects using control functions in models with continuous, endogenous treatment and heterogeneous effects. *Econometrica, Econometric Society pages*, 76(5):1191 – 1206.

Fujita, S. (2011). Effects of extended unemployment insurance benefits: evidence from the monthly cps. *Working paper*.

Heckman, J. and Singer, B. (1984). A method for minimizing the impact of distributional assumptions in econometric models for duration data. *Econometrica*, 52(2):271–320.

Imbens, G. and Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies. The Review of Economic Studies. The Review of Economic Studies*, 79:933–959.

Katz, L. F. and Meyer, B. D. (1990). The impact of the potential duration of unemployment benefits on the duration of unemployment. *Journal of Public Economics*, 41:45–72.

Kroft, K. and Notowidigdo, M. J. (2011). Should unemployment insurance vary with the unemployment rate? theory and evidence. *NBER Working Papers*.

Landais, C. (2013). Assessing the welfare effects of unemployment benefits using the regression kink design. *IZA Discussion Papers*.

Lee, D. S. and Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature, American Economic Association*, 48(2):281–355.

Meyer, B. D. (1990). Unemployment insurance and unemployment spells. *Econometrica*, 58:757–82.

Mitman, K. and Rabinovich, S. (2015). Optimal unemployment insurance in an equilibrium business-cycle model. *Journal of Monetary Economics*, 71(99-118).

Moffitt, R. (1985). Unemployment insurance and the distribution of unemployment spells. *Journal of econometrics*, 28:85–101.

Mortensen, D. T. (1977). Unemployment insurance and labor supply decisions. *Discussion Papers*.

Nielsen, H. S., Sorensen, T., and Taber, C. R. (2010). Estimating the effect of student aid on college enrollmentestimating the effect of student aid on college enrollment: Evidence from a government grant policy reform. *American Economic Journal: Economic Policy, American Economic Association*, 2(2):185–215.

Powers, D. A. and Xie, Y. (2000). *Statistical Methods for Categorical Data Analysis*. San Diego: Academic Press.

Rothstein, J. (2011). Unemployment insurance and job search in the great recession. *Brookings Papers on Economic Activity, Economic Studies Program, The Brookings Institution*, 43(2 (Fall)):143–213.

Schmieder, J. F., von Wachter, T., and Bender, S. (2012). The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over twenty years. *Quarterly Journal of Economics*, 127(2):701–742.

Simonsen, M., Skipper, L., and Skipper, N. (2010). Price sensitivity of demand for prescription drugs: Exploiting a regression kink design. *Economics Working Papers*.